EXPERIMENTAL PSYCHOLOGY

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EXPERIMENTAL PSYCHOLOGY

A Manual of Laboratory Practice

BY

EDWARD BRADFORD TITCHENER

VOLUME I

QUALITATIVE EXPERIMENTS

PART II. INSTRUCTOR'S MANUAL

Sobald man einmal die Seele als ein Naturphänomen und die Seelenlehre als eine Naturwissenschaft auffasst, muss auch die experimentelle Methode auf diese Wissenschaft ihre volle Anwendung finden können. — WUNDT.

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To My Friends

FRANK ANGELL, MAX VON FREY, AUGUST KIRSCHMANN, OSWALD KÜLPE AND ERNST MEUMANN

PREFACE

THE general purpose with which this book has been written is sufficiently indicated by its title. I have selected a number of the 'classical' experiments of Experimental Psychology, and have tried to present them in such a way that their performance shall have a real disciplinary value for the undergraduate student. Within this general purpose, my aim has been twofold. I have sought to show, in the first place, that psychology is above the laboratory: that we employ our instruments of precision not for their own sake, but solely because they help us to a refined and more accurate introspection. And secondly, just as in my *Outline of Psychology* and *Primer of Psychology* I gave the results of experimentation a prominent place in the psychological system, so here I have treated the selected experiments not as separate exercises, but as points of departure for systematic discussion. I hope that the book may find its sphere of usefulness. I sorely felt the need of some such guide when I entered the Leipzig laboratory, and I have felt it as sorely throughout my teaching experience. It is needless to add that, although eight years have gone to its making, the Manual falls lamentably short of its ideal. A book in conception is a perfect piece of workmanship: the book that leaves the author's hands is but a rough approximation to the first design.

My greatest debt, here as elsewhere, is to Wundt. I was impelled towards experimental psychology by dissatisfaction with the logical constructions of the English school; and it was Wundt who taught me the essential lesson of systematic introspection. If my recent writing has seemed rather to be directed against Wundtian doctrines, that is but the natural

reaction of a pupil who cannot swear to the literal teaching of the Master. Next to Wundt I have gained most from the work of Hering and Stumpf.

It is a pleasant duty to acknowledge the assistance that I have received in the preparation of the Manual. My wife has shared the labour of the book from its beginning. I am indebted to my sister, Miss L. C. Bedlow, for drawing most of the Figures of Part I. My colleagues, Dr. I. M. Bentley and Dr. G. M. Whipple, have given freely of their time and counsel. For help upon many special points I have further to thank Professors B. G. Wilder and S. H. Gage, of Cornell University, Professor E. C. Sanford and Mr. L. N. Wilson, of Clark University, Professor J. McK. Cattell, of Columbia University, Professor W. B. Pillsbury, of the University of Michigan, and Dr. E. A. McC. Gamble, of Wellesley College.[1] Last, not least, I must set here the name of John Winslow, — good man, true friend and wise physician, — to whose scientific comradeship during the past eight years I owe more than I can well express, and whose recent death has brought to me, as it has brought to many others, a sense of irreparable loss.

Cornell Heights, Ithaca, N.Y.,
November 1, 1900.

[1] The Manual is a product of the laboratory, and embodies the work of a long roll of students. I can mention but a few names here. But I cannot omit a word of thanks to Miss L. Aldrich, late Scholar in Philosophy; to Dr. W. C. Bagley, of the Department of Psychology; to Dr. E. I. Conant, of the New York Normal College; to Mr. I. MacKay, Fellow in Philosophy; to Miss M. E. Schallenberger, of the San Francisco State Normal School; and to Dr. T. L. Smith, late Honorary Fellow in Psychology.

TABLE OF CONTENTS

INTRODUCTION

HINTS TO THE INSTRUCTOR

PART I

SENSATION, AFFECTION, ATTENTION AND ACTION

CHAPTER I. VISUAL SENSATION

CHAPTER II. AUDITORY SENSATION

CHAPTER V. OLFACTORY SENSATION

CHAPTER VI. ORGANIC SENSATION

CHAPTER VII. THE AFFECTIVE QUALITIES

CHAPTER VIII. ATTENTION AND ACTION

PART II

PERCEPTION, IDEA AND THE ASSOCIATION OF IDEAS

CHAPTER IX. VISUAL SPACE PERCEPTION

CHAPTER X. AUDITORY PERCEPTION

CHAPTER XI. TACTUAL SPACE PERCEPTION

CHAPTER XII. IDEATIONAL TYPE AND THE ASSOCIATION OF IDEAS

APPENDICES

INDEX OF FIGURES

INTRODUCTION: HINTS TO THE INSTRUCTOR

§ 1. **Conduct of the Course in General.** — This Course aims at two things: first, and more especially, to teach the student to psychologise, and secondly to acquaint him with the most reliable methods and most securely established results of experimental psychology. Information concerning methods and results can be obtained, without much trouble, from the text-books. But introspection cannot be learned from books. If one is a born psychologist, it may be learned from the experience of ordinary life; and learned the more quickly, if this experience is supplemented by reading and by listening to lectures. As a general rule, however, and to the average student, an understanding of the introspective method either comes by way of the laboratory or does not come at all. It is apt to come somewhat suddenly, after a longer or shorter period of blind work. It comes under all sorts of circumstances: an experiment that is routine drudgery to one man may be illuminating to another, just as a certain form of words in an argument or proof may leave one hearer untouched while it carries conviction to another.[1] Once gained, it is never lost: one can no more forget how to introspect than one can forget how to walk or swim.

On the other hand, there is no guarantee that introspection shall be learned in a course of laboratory practice. Rules may be applied and definitions memorised; examinations may be passed and, for that matter, investigations made; while the student is still wholly innocent of the introspective attitude. Nor is there any guarantee that, when learned, it shall always be used aright. We may introspect inadequately, as we may swim snatchily or walk slouchingly. It is, then, of the very greatest

[1] E. Hering, *Zeits. f. Psych.*, i., 1890, 19.

importance that the Instructor shall teach from introspective knowledge, and not from book knowledge only. No one denies that acquaintance with the methods and results embodied in the monographic literature of the science is essential for the psychologist, whether he be teacher, investigator or student. But for the conduct of an experimental drill-course, introspective familiarity with the processes which are to be examined is the one condition of good work. If time allows, therefore, each experiment should be performed by the Instructor, both as *E* and as *O*, before it is performed by the members of his class. In any case, however, the experiment should be gone through in shorthand, — all the manipulations made, and the method followed out briefly in all its turns and through all its stages, — and enough introspections taken to furnish a control of the statements of the text. The Instructor should know, by experience, just how accurate a colour equation can be, and just how the sensation from a warm spot on the skin 'feels': he should not seek to acquire this knowledge during class hours.

On the other hand, it is probably safe to say that no single psychological experiment has as yet been worked through to the bitter end. The science is young: Wundt's laboratory at Leipzig, the oldest foundation of its kind, is on the eve of its twenty-first birthday as the author pens these lines. And the problems are so numerous and so patent that investigators have been tempted to range widely rather than to concentrate their energies upon single issues. An intelligent student will ask many questions in the course of the year to which experimental psychology has no answer ready. Some of these questions will take the form of minor problems which, in certain cases, may with advantage replace the set experiments of the text.

§ 2. **Qualitative Work.** — The experiments of this Volume are termed 'qualitative,' and emphasis is laid throughout upon 'qualitative' as distinct from 'quantitative' work. It is important that the Instructor accurately understand the meaning of this distinction. It does not mean, in the first place, that the experiments are rough and inexact, mere approximations to the 'quantitative' ideal, indefinite preliminary tests which further work shall

make definite. Nor does it mean that the methods employed are rough and inexact, incapable of formulation in numerical terms. There are, it is true, cases in which the experiments of the text are less exact than could be wished, and in which the methods are incompletely worked out: these defects are, however, due to exigencies of materials or time, and are in no way inherent in the qualitative procedure. What the distinction implies is rather this: that the student's attention is directed not to the 'How much?' or the 'How well?' of mental function, but to the 'How?' of mental structure. The problems: How quickly can one idea call up another? how many impressions can be attended to at once? how small a difference of colour-tone can be perceived? are quantitative problems. The questions: How does one idea call up another? what is going on in the calling-up consciousness?—What happens when I am attending-to a number of impressions at once? how do the impressions thus attended-to differ from other impressions? what are the characteristic processes of the attentive consciousness?—What does 'to perceive as different' mean? what is the process of comparison, of the judgment of difference? what are the contents of the 'just different' consciousness?—these are qualitative questions.

It is, however, clear that in experimental psychology the terms 'qualitative' and 'quantitative' are not mutually exclusive. If the work is qualitative, it still must be accurate; and accuracy will mean the introduction of measurement, of quantitative formulation, at various stages of the experiment. The qualitative course of an after-image is not fully described unless the series of time-values run alongside of the colour changes. We cannot map our field of smell aright unless we set a time-limit to the stimulation. We cannot turn our introspective data to account for the 'How?' of sound localisation unless we have the errors and the directions of error expressed in numerical terms. On the other hand, if the work is quantitative, it must still be supplemented by qualitative introspection, or the figures and formulæ are barren. Reaction times are worth very little without the accompanying analyses of the action-consciousness; knowledge of the range or limits of an optical illusion is worth very

little without a qualitative analysis of the factors upon which the illusion depends. And so on.

We may, then, modify our former statement a little. In qualitative work, we may say, the student's attention is directed primarily upon the 'How?', and the 'How much?' comes into account only when and in so far as it helps towards a more exact characterisation of the 'How.' In quantitative work, the student's attention is directed primarily upon the 'How much?', and the 'How?'—though it must be taken constant account of—is regarded only in so far as it throws light upon the answer returned by consciousness to the 'How much?'. The difference will work out, in the concrete, to mean that in qualitative work the methods most generally followed are those of exploration, the mapping of a sense field, and of analysis and synthesis, the tearing-apart of a complex into its constituent elements, and the putting-together of the elements to form a known complex; whereas, in quantitative work, the methods most generally followed are the 'psychophysical measurement methods' (gradation and error) and the chronometric methods. Again, however, the line of division is relative only; the author has introduced the simple reaction into this part of the Course, as a qualitative experiment, and has employed, *e.g.*, a simple form of the method of minimal changes in an experiment upon tactual localisation.

There can be no doubt that the atmosphere and spirit of the psychological laboratories have changed very greatly during the last ten years. One has only to compare, *e.g.*, the work of E. Meumann on the time consciousness with the earlier work proceeding from the same laboratory and published in the same journal, to see how radical this change of aim and emphasis has been. The old quantitative standpoint is now almost forsaken, and qualitative analysis not only has a literature of its own, but is coming to play a larger and larger part in the investigations described above as quantitative.

The change has been general, as movements of tendency in science are likely to be, and all the leading laboratories have done their share towards bringing it about. If, however, we are to indicate a single man, as representative of the qualitative point of view, we cannot hesitate to name G. E. Müller. In all Müller's work we find, alongside of a consummate mastery of quantitative method, a strenuous insistence upon qualitative analysis: *cf.*, *e.g.*, the recent monograph by L. J. Martin and G. E. Müller, *Zur Analyse der*

Unterschiedsempfindlichkeit: experimentelle Beiträge. Leipzig, Barth, 1899. Mk. 7.50.

The author may add that he insisted on the importance of a 'qualitative attitude' to the students who took this Course for the first time in 1892, and that experience has simply confirmed the views which he then held.

§ 3. **The Preparation of the Instructor.** — The foregoing Section raises a question that has been much debated among teachers of psychology: the question whether psychology can be taught and learned without a pretty thorough previous preparation in physics, mathematics and physiology.

It stands to reason that the director of a laboratory (or his assistant or mechanician) must be familiar with physics, or at least with certain parts of physics, if the work of the laboratory is to go on at all. Tuning-forks and pendulums and induction coils are, in the first place, physical instruments, and must be understood as physical instruments if they are to be used in the teaching of psychology. If the laboratory is not only a 'teaching' but also a 'research' laboratory, a good knowledge of physics on the part of its director becomes imperative.

With regard to mathematics the case is a little different. Two statements by J. McK. Cattell, made in the course of the same year, are interesting. "Perhaps we [men of science] should all know how to use a tool as fine as the calculus" (*Science*, Feb. 4, 1898, N. S., vii., 153): and "The calculus may ultimately become important in psychology, but as yet no one has accomplished much by its application" (*Psych. Rev.*, v., 1898, 658). Except that he would make the second sentence somewhat more hopeful, the author fully endorses these two remarks. Knowledge of elementary mathematics is part of a man's general scientific outfit: but one may work a lifetime, and with success, in psychology, without needing the knowledge.[1]

A general knowledge of the architecture of the brain and of the functions of the nervous system, and a somewhat more

[1] It is, of course, — and the remark is so obvious that the author would hardly venture to make it, were it not justified by certain recent publications, — nothing less than fatuous to pretend to a knowledge of mathematics if one does not possess such knowledge; to introduce into one's work symbols and equations which are not a part of one's ordinary mental furniture.

special knowledge of the structure and functions of the sense organs, are essential; while, for certain kinds of research, a very thorough knowledge of physiology is required. We have, fortunately, two good outlines of those portions of physiology which the psychologist must know in Wundt's *Phys. Psychologie* and Ebbinghaus' *Psychologie;* James' treatment of the subject, in the *Principles of Psychology*, covers only a part of the ground. At the same time, the student must be led clearly to understand that nerve physiology is not psychology. As Höfler says: "There is no road that leads to psychology from metaphysics. But neither is there any road to psychology from physiology." Where it is necessary, in a psychological laboratory course, to take time for physiological matters, — as will probably be the case, *e.g.*, in the study of visual space perception, — the Instructor should sharply distinguish between the physiological preliminaries and the psychological problem.

Students who enter the psychological laboratory with little or no physiological training are very likely to manufacture a physiology when called upon to explain a psychological fact. "You say that I touched you on the back of the hand. How do you know that I touched you there?" — "Why, I suppose there is one set of nerves for the back of the hand that is different from the other sets of nerves." This sort of physiological 'supposing' in psychological work is familiar to all teachers of experimental psychology. It must be promptly and finally suppressed.

We conclude, then, that it is the business of the Instructor to know something of physics and physiology, — not to teach them, by any means, but to have his psychological teaching infused with them. If he know something of mathematics, so much the better; and if he know something of history, and literature, and general biology, so much the better. Always, however, he must be careful to make it clear to the student where these other sciences end and the science of psychology begins.

§ 4. **The Preparation of the Student.** — The present Course implies a preliminary course of lectures on general psychology, such a course, *e.g.*, as is laid down in the author's *Outline of Psychology*. Alongside of this preliminary course should run courses in general physics and general physiology (with laboratory work), and in French and German. Special work in nerve

physiology and in physics (light, sound and electricity) should be taken concomitantly with this Course. The author has found it advisable, in this second year of work, to give a short reading-course in French and German psychology, a technical supplement to the general language courses. Finally, the laboratory drill should be followed by a year's lecture course in Systematic Psychology, during which the student works over for himself, at first hand, certain portions of the monographic literature. Laboratory work — practice or the investigation of questions arising in the course of practice — is still continued. After these three years of training come the work of the Seminary, and research proper.

The qualities that make a successful student have been enumerated in the text. It is part of the Instructor's business to cultivate these qualities in natures that already possess them, and to induce them — so far as they can be induced by training — upon temperaments to which they are naturally foreign. And this means that the Instructor must make a study of *mental types*. A few words may be said upon the topic here.

The students that enter the laboratory may be classified roughly as 'objective' and 'subjective' in type. L. W. Stern (*Ueber Psychologie der individuellen Differenzen*, Leipzig, 1900, 99 ff.) gives the following description of the two natures. The objectively-minded observer "yields himself as passively as possible to the impression, is of a contemplative turn, follows with great exactness the variations of the external stimulus; he delays his reaction until he has arrived at a confident judgment." The subjectively-minded, on the contrary, "is constantly expecting something, is easily led to react, by prepossession or impatience, before he has attained to full certainty by means of perception pure and simple; he has in general a strong tendency to motor discharge; the moment of perception is determined not so much by the nature of what is perceived as by a subjective periodicity (rise and fall) of psychical activity." T. L. Bolton (*Amer. Journ. of Psych.*, vi., 1893, 208 ff.) gives a classification which, by a little rearrangement, may be squared with this. The objectively-minded students, he says, "take a mod-

erately critical attitude. They are concerned in others' opinions in so far only as other opinions suggest different points of view. They give their own opinions when they have considered all the phases of the experiment that are suggested to them. They are unconcerned about the outcome of the experiment. They are not dogmatic; they might have a different opinion under different circumstances or with further consideration. In the light of the evidence before them, they hold to a certain view." The subjectively-minded fall into two groups. "The first includes those persons who yield immediately to any suggestion that is offered. This attitude results, then, from a social practice. In society, people do not wish to antagonise others. They instinctively give assent to any opinion. In an experimental investigation, if the operator will just give the slightest hint of his theory or preference, they will add the weight of their opinions. If the operator leads them into giving an opinion which is opposed to his theory, 'consistency becomes a jewel'; they stick to their opinion stoutly. If the experiment shows plainly that they are wrong and it is preposterous to hold such a view, they make a compromise with their former position, and try to excuse themselves for having been led astray. They remain respectfully silent afterward and avoid, if possible, giving an opinion. If they are forced to make a judgment, they do it tentatively; they are not sure. Of a number of possible views they cannot make up their minds which is the correct one. They generally hair-split until they find out some one's opinion, and then agree with that." The second "class includes those persons who are excessively critical. They incline always to an opposite view. The experiment is not conducted properly to suit them; they are not in their best mood for judgment. They are sure to take ground against some one's opinion. If they cannot get any clue to others' opinions they are doggedly silent or quibble, and refuse to answer except they qualify their answers to such an extent that the answer means nothing. This class of subjects is intellectually dishonest. If they are compelled to answer, they indulge in hair-splitting differences between their opinions and those of some others."

Bolton, writing as investigator and not as teacher, passes

severe judgment upon the subjective type. And it is true that there are persons who, from laziness, from some form of incapacity, or from ingrained prepossession, are unfitted for psychological work. If such persons find their way into the laboratory, however, they find their way out again, in the course of the first fortnight. On the other hand, we rarely, if ever, meet with a pure type; the rule is that the objective or the subjective attitude is dominant in the total character. Most natures are sufficiently objective to afford a foothold to training; and for the student who is willing to see the matter through, training will accomplish wonders. Set a man to work for a year: hold him strictly to the work, insist that he be thorough: show him his faults unflinchingly, in all their glaringness; at the same time, work with him, sympathetically, as ready to encourage as to blame: fit your praise and blame alike to his character and disposition: and, though you have not changed his nature, you will have wrought a very considerable change in his methods and attitudes. Few, if any, of the students who take this Course in a given year will become psychologists. But the Instructor can effect this much, — that all, when they leave the laboratory at the end of the year, carry away with them, besides some little knowledge of experimental psychology, the habit of concentrated and continuous work.

No means must be neglected that will assist towards this end. Cautions and injunctions in plenty are given in the text. But cautions and injunctions, even if understood, are not necessarily effective. It may be taken for granted that the student who comes into the laboratory for the first time, whatever his preparation may have been, does not realise how much he has to unlearn, how great is the gulf that divides popular from scientific psychology. Moreover, the student of psychology, as we have him, has too often had no laboratory training of any sort. The author has, therefore, been accustomed to supplement the directions of the text by painting the reverse of the picture. No offence will be given (and the giving of offence would defeat the whole object of the Instructor) if the situation is handled tactfully and good-naturedly; and a great deal may be accom-

plished, at one stroke, that could otherwise have been attained only slowly and with difficulty.

How to fail in Laboratory Work

(1) Assent readily, and with an air of complete intelligence, to all that the Instructor says. Make no effort to understand his explanations yourself, but trust to your partner for the conduct of the experiment.

(2) Do not accept any general explanation, under any circumstances. Cherish the belief that your mind is different, in its ways of working, from all other minds, and that you must be individually treated.

(3) See yourself in everything. If the Instructor begin an explanation, interrupt him with a story of your childhood which seems to illustrate the point that he is making. If he is formulating a law, interrupt him with an account of some exception that has occurred within your own or your friends' experience. Go into the minutest detail. If the Instructor incline to reject your anecdotes, argue the matter out with him in full.

(4) Call upon the Instructor at the slightest provocation. If he is busy, stroll about the laboratory until he can attend to you. Do not hesitate to offer advice to other students, who are already at work.

(5) Look very critically at the instruments that are put into your hands. Point out their defects to the Instructor, and suggest improvements. Offer to spend the next few laboratory hours in the workshop, getting out a better appliance.

(6) Never lose sight of the greater questions of the science in the petty routine of experimentation. If, *e.g.*, the Instructor is explaining the use of the campimeter, ask him whether experimental psychology is not materialistic in tendency, or if he thinks that the results of experimental psychology are of value for education.

(7) If you are balked by an introspective problem that your partner has solved, either say that of course you had thought of that, but that it seemed too trivial to mention, or fall back upon the uniqueness of your mental constitution. Tell the Instructor that the science is very young, and that what holds of one mind does not necessarily hold of another. Support your statement by anecdotes.

(8) Work as noisily as possible. Converse with your partner, in the pauses of the experiment, upon current politics or athletic records. Get thoroughly roused up and excited before you proceed with your work.

(9) Do not take the work seriously. Explain frankly, when you enter the laboratory, that you have no belief in the methods and results of experimental psychology, but that you like to know what is going on in the various departments.

Or, as an alternative rule: Explain, when you enter the laboratory, that you have long been interested in experimental psychology, and that you are overjoyed to have found the present opportunity of studying it. Describe

the telepathic experiences or accounts that aroused your interest; ask the Instructor if he has read so-and-so's recent paper in so-and-so, and express disappointed surprise when he replies (as he will) that he has not.

(10) Make it a rule always to be a quarter of an hour late for the laboratory exercises. In this way you throw the drudgery of preliminary work upon your partner, while you can still take credit to yourself for the regularity of your class attendance.

The author has never found the paragon who obeyed all these precepts. Diligent attention even to one or two of them will, however, be enough to secure the failure required.

§ 5. **Special Directions.** — A word may be added upon some special points. (i) The Instructor should be careful not to suggest, by word or manner, that a certain result is expected from a certain experiment. Students in the early stages of training are exceedingly suggestible. The Questions appended to the experiments of Part I have been chosen with this fact in mind. They may, however, have a suggestive influence. If the Instructor has any suspicions on the matter, he should find an occasion to insist to the student that the Questions are wholly objective and neutral in character, and that a negative answer is as likely to be right as a positive. (ii) The "Related Experiments," briefly outlined below, should be worked out (if they are to be performed at all) as carefully and under as strict conditions as the regular experiments of the Course. (iii) The Instructor should see that the note-books are 'written up' at frequent intervals. It is not only that the work soon accumulates. A student is compelled, if he is writing a week after the event, to think himself back into the conditions of the experiment; he has lost the freshness and reality of the experience; and his record suffers in consequence. (iv) The author has said in the text that introspections should be definitely and concretely worded. A sharp distinction must be drawn between concrete phrasing and picturesque or pictorial phrasing. The more concrete the report, the more closely does it tally with the experience. But a picturesque report — a report which may seem, at first reading, to reproduce the warmth and intimacy of the experience as no other form of words could do — will generally be found, on deeper study, to rest upon some

superficial analogy, and to contain no more of real introspection than the most arid and abstract sentence. Impressionism is as bad as formalism; what is wanted is photography. (v) It is not advisable, even if the resources of the laboratory permit, to set the whole class to work upon the same problem. The student should be given his choice of a beginning upon sight, sound, smell, taste or touch. If he has no choice, the initial experiments should be distributed as evenly as possible over these five departments. The work of the Instructor is thus made more varied and less monotonous; the students are impressed with the fact that competition plays no part in the psychological laboratory; and interest is aroused and kept alive by the exchange of experiences outside of the laboratory.

The apparatus figured in the text are the cheapest and simplest pieces that the author has found reliable. They are not necessarily the best; but they are the best to be obtained at small cost, and they are adequate to the experiments. The prices affixed are approximate only; the cost of a particular piece may vary from year to year with the demand, the cost of materials, the addition of improvements, the simplification of parts, etc.

The apparatus figured in this Part are, in general, either historical pieces, which have played a definite part in the development of the science, or standard pieces, the best of those that can be ordered ready-made from the instrument-makers. Many of the latter class have their defects. All alike, however, are instruments with the structure and working of which the student should be familiar. The author would therefore advise that large wall-diagrams (or lantern slides), properly lettered, be prepared from the cuts, and the use of the apparatus explained in lecture at the conclusion of the experiments in which their simpler counterparts have been employed. Other diagrams may be prepared from the cuts in instrument-makers' catalogues, in the technical journals, and in the illustrated Psychologies. The author has sought to give, in every case, a reference to the book or paper in which the instrument is described.

The Results quoted are copied from actual laboratory records. They show what may be done by a student of good average

ability who takes his work seriously. They are not to be compared, in any sense, with the results of investigations published in the psychological journals. The author prints them for the reason that (with the very few exceptions mentioned below) there is nothing in the literature to set the standard of attainment in experiments of this kind.

The literary references are very incomplete, — although both Instructor and student may object to their range and number. The author has never quoted for the sake of quoting; there is something of value for the experiment in every one of the monographs cited. The books required for answers to Questions are all readily accessible.

All the experiments of this Volume offer opportunities for the discussion of points in systematic psychology. The author has introduced some such discussions, approximately in the same measure and with the same fulness that he has found practicable in actual laboratory work. The body of the Course is, however, neither dependent upon nor necessarily connected with any particular system. The materials here given may be utilised for lectures or for informal laboratory conversations; the author's views may be accepted or simply made the basis of criticism; the references may be looked up by the student for himself or by the Instructor for him; systematic matters may be given more attention than the author has given them or may be entirely ignored. Such things lie, of course, in the discretion of the Instructor.

Finally, it may be said that this first Volume is intended to represent a half-year's work for third year students, the second half of the year being devoted to the quantitative work of Vol. II.

§ 6. **Courses in Experimental Psychology.** — There are at the present time three published Courses (or part-courses) in laboratory psychology.

The most complete Course is the *Psychologische Schulversuche, mit Angabe der Apparate*, by A. Höfler and S. Witasek (Leipzig, Barth, 1900; pp. viii., 30). This little book outlines seventy-five tests or experiments for use in the Austrian *Gymnasien*.

In choice and arrangement of material it follows Höfler's *Psychologie*. An especially good feature is the first-hand reference to authorities (Fechner, Stumpf, Hering). On the other hand, as the dimensions of the work attest, practically nothing is said of the conduct of the experiments, of sources of error, etc.

An Elementary Course in Psychological Measurements, by E. W. Scripture (Studies from the Yale Psychological Laboratory, iv., 1896, 89–139), describes seventeen of the thirty experiments which constitute the elementary course in the Yale laboratory. In most cases, specimen records are given. Emphasis is laid upon manipulation and computation, while introspection is practically ignored.

More akin to the present Course is E. C. Sanford's *A Course in Experimental Psychology:* Part I., Sensation and Perception (Boston, Heath, 1898; pp. viii., 449. The first six of the nine chapters of the work were issued as 'advance sheets' in 1894). This book outlines no less than 239 experiments, qualitative and quantitative, upon the subjects mentioned in its title. It has a high historical importance, as the first manual of experimental psychology; it has exerted, and still exerts, a wide influence, as the gateway through which American students are introduced to laboratory work; and it is a monument of accurate erudition.

The author's indebtedness to Sanford is very great, both on positive and on negative grounds. The investigator who goes over the literature of the science for the first time is undertaking a labour which, if conscientiously performed, is in large measure spared to later workers in the same field: and Sanford's 'Course' is alike remarkable for range of knowledge, impartiality and judgment in selection. But a pioneer work must pay the penalty of its originality. And, from the educational point of view, the 'Course' appears to the author to have three defects. (1) The brevity of its directions is likely to give the student an idea that the psychological experiment is intrinsically easy, and capable of very rapid performance. (2) Its neglect of introspection tends to obscure the essential difference between the psychological experiment and the experiments of the natural sciences. (3) It throws too great a burden upon the Instructor. As a *catalogue raisonné* in the hands of one thoroughly familiar with its subject-matter, the book is admirable; if this familiarity be lacking, the Instructor may be led into the same error to which the student is liable. There is, indeed, some evidence that Sanford has himself become sensible of these defects as his work proceeded; for not only are the later

sections written in much greater detail than the earlier, but the latest of all contain illustrative records of results.

Mention should be made here of H. Münsterberg's *Pseudoptics* (Milton Bradley Co., Springfield, Mass.; $5.00), a set of portfolios containing materials for a good number of experiments in visual sensation and perception.

The experiments of the text have been chosen from the much greater number that have been worked out in the Cornell laboratory. The author does not regard their selection as final, even for his own purposes. In some cases, as in the spheres of visual and cutaneous sensation, the choice of problems presents no difficulty; in others, as in auditory sensation, it is far from easy. Moreover, it is true in general, as Sanford says, that "what a good laboratory course ought to include is not yet wholly clear." At the same time, the range of experiments that afford training in laboratory arts, that give opportunity for introspection in the various fields of mind, and that can be performed with some sort of thoroughness in the brief time at the student's disposal, is not so wide as at first thought it might appear. Thus, experiments upon the more complex processes or consciousnesses (memory, imagination, reasoning, emotion and the like) are, for the most part, ruled out of a Manual by the time limit; they require systematic work, preceded by a term of practice, and so take on the form of investigations rather than of single experiments.

PART I

SENSATION, AFFECTION, ATTENTION AND ACTION

CHAPTER I

VISUAL SENSATION

§ 7. **Sensation.** — There has been a good deal of discussion, of recent years, as to the right way to teach psychology. Ought one to begin with the 'real' mind, and work down from that to sensation, travelling in this way from the better known to the less known? Or ought one to begin with the simplest, and work up to the most complex, — to begin with sensation, and end with mind?

Psychology is too young a science, — nay, for that matter, natural science itself is perhaps too young, — to permit of our hoping for any final settlement of the issue. Moreover, it is worth while to remind ourselves explicitly that its settlement is not a life-and-death affair. If the teacher knows his subject, and is in love with it, it will in large measure teach itself: whatever the formal setting of the teaching, the student will imbibe the scientific spirit, and learn to think in scientific terms. However, since a choice must be made, and has been made in this book, we may dwell for a moment upon the practical reasons for choosing.

In a course of general and elementary lectures, such as a Manual like the present presupposes, it is, no doubt, necessary to begin with the 'real' mind. One cannot start a train of thought without having a starting-point. And it is well, in such a course, to give illustrations of the analytic procedure. The steps in the procedure itself will hardly be understood: if

for no other reason, because it would take the lecturer too long to explain just why the investigator did this and did that; the experiment implies the science. Still, the hearer will have a general idea of the first problem of psychology. Even here, however, the influence of popular psychology is so strongly antagonistic to clean psychological thinking, and the hearer's furniture of ideas is such a mixed medley of psychological tradition, logical construction, and ethical appreciation, that it becomes imperative, from time to time, to break away from the starting-point: to work from the known to the unknown, not by gradually educing the implications of the known, but by confronting it point blank with a statement of the unknown.

If this holds of an introductory lecture course, it holds still more of laboratory work. The student who enters the psychological laboratory is burdened with a mass of misconceptions. The physics and physiology of the 'average man,' crude and foolish as they are, come much nearer to fact than his psychology does. Scientific psychology is hardly older than he is himself, whereas the race has been speculating upon mental things for more than two thousand years, and the transmitted speculation has become dogma. • Hence it seems pedagogically desirable that the student should be asked, from the first, to put himself in a new attitude to mind; to hold his opinions as to mental function and faculty in abeyance, until he has become familiar with the results of scientific analysis, and has seen mind take scientific form from the synthesis of the elements. And it then becomes a matter of method, of time-saving, that he should begin with the simple and be gradually led on to the complex.

Further: it must not be supposed that this direction of work holds the student to a rut, aside from the wider issues of general psychology. The facts of sense psychology are not blind observations, made and done with. Laboratory work, intelligently conducted, cannot fail to raise the main questions of the psychological system: only it raises them locally, so to say, and in concrete form, instead of generically, in conceptual phrasing. When, therefore, the student comes to the historical and systematic work which should follow the laboratory drill, he finds

that his old opinions have been insensibly modified: the problems of mind have received a new formulation, in which they show clear of any logical or ethical colour: a critical judgment — weak at first, perhaps, but still critical — is brought to bear upon the inherited axioms and facile generalisations of popular psychology.

And this result could not have been obtained, if we had worked from the 'real' mind downwards? Certainly, it could: that we have admitted. But it could have been obtained only under disadvantages, and with waste of time. Under disadvantages, — for there has been no resolute effort to substitute the scientific for the naïve view of mind, and so the start is bad: with waste of time, — because your analysis of any complex process, undertaken for the first time, will leave you with loose ends of process, unaccounted for, which may be elemental or may not: you cannot tell, until you have analysed other complexes, and then repeated your analysis of this: and even so, you are bound to test the validity of your work by doing precisely what the opposite method does, working from below upwards, from your professed simples to the actual compound. Unless, then, there is training, otherwise unobtainable, in work done by this method, the method would seem to be pedagogically inferior; and it is argued in the text that no such training is afforded.[1]

The following points in the psychology of sensation should be laid clearly before the student from the outset.

(1) The sensation *is* strictly subject-matter for psychology. This point can be variously brought out. Thus, physics deals with light, ether waves; physiology with the working of the eye; psychology with colour and brightness. Or again: the world of natural science is colourless and soundless, —

[1] The above discussion has taken the proposal to begin psychology with the 'real' mind at its own estimate: in other words, has accepted the possibility of such a beginning. It does not require much thought, however, to see that no sort of psychology can begin with the 'real' mind. Science cannot deal with the individual, but only with the abstract, the generally valid. Whether we begin our psychology with 'sensation' or with a case of 'association of ideas,' we are always beginning with an abstraction. The difference between the two methods of teaching is a difference in degree, not in any sense a difference in kind.

not in the sense that it is dark and silent, but in the sense that any word connected with seeing and hearing is foreign to it, makes nonsense with it, as it would be nonsense to talk of a benevolent carpet.

(2) The sensation is not 'simple' in the sense that it is characterisable by a single adjective. It is an ultimate process of structural analysis, as the cell is in morphology or the 'element' in chemistry. But cells have a highly composite structure, while they differ largely as regards shape, size, length of life, etc. ; and the chemical elements, though irreducible as 'gold' or 'iron,' still show differences of physical attributes. The sensation gives no indication of constituent parts, as the cell does ; but it has various aspects or properties, — of which only 'quality' is to be dealt with here.

(3) This sensation, which is a structural unit of the adult mind, is not the genetic unit of mind at large. Mind has not grown by aggregation of sensations, by the simple addition of our 'blue' and 'yellow' to a given 'black' and 'white' that are also like ours, and by the further addition, still later, of a ready-made 'red' and 'green.' Even granted that we could analyse the primitive mind into sensations, still its 'black' and 'white' would be so different from our own as to be hardly recognisable. At the time when the heart begins to pulsate, there are no muscle fibrillæ in the myocardium ; we have the sight of purely protoplasmic, undifferentiated cells making strong rhythmical contractions. If, now, the structural elements of the primitive mind are 'sensations,' they are sensations only in the sense that these primitive heart-cells are 'muscle' cells.

(4) Nor is this sensation a functional unit of mind. It is not the tiniest bit of mind that can give us knowledge, not the simplest form of knowing. Knowing, *i.e.*, does not come, any more than does mind, from the addition of sensation to sensation. The sensation 'blue' does not tell us of a blue object, does not even tell us that it comes from the eye. It simply presents itself, as a mental irreducible ; and we have to examine it for its own sake, — to watch its behaviour under varying conditions, and to trace it in all the compounds into which it enters. If the student insist, as at first he may, that he cannot possibly think of a 'blue' that is not a 'blue something,' the answer is twofold. He is not to 'think of' a blue at all, but to be a blue ; his consciousness is to be a blue-consciousness, not a consciousness made up of ideas associated to blueness in the course of his experience. And the reason for his difficulty is simply that he has not pushed his introspection far enough ; he has not stripped the sensation 'blue' of all the overlay of associated (more especially organic) processes that make 'blue' mean 'the blueness of something' in everyday life.

(5) The sensations which we begin with, in our work, are precisely the same as the sensations that we speak of as entering into perceptions, associations, etc. Thus the 'pressure sensation' of the pressure spot is the same sensation as the 'pressure' that we deal with when we are attacking the problem of cutaneous localisation. There is no gap, and therefore no need of a bridge, between sense psychology and the psychology beyond sense.

It is not to be supposed that these points will all be grasped, or their significance realised, in a week or a month or even a term. But opportunities for insisting upon them will be constantly arising in the course of experimental work, and every such opportunity should be seized.

§ 8. **Visual Sensation.**— On visual sensation in general see:

H. Aubert, Grundzüge d. physiol. Optik, 1876, 479.
H. Ebbinghaus, Grundzüge d. Psychol., i., 1897, 180.
A. Fick, in Hermann's Handbuch d. Physiologie, iii., 1, 1879, 139.
H. L. F. von Helmholtz, Handbuch d. physiol. Optik, 1896, 231.
E. Hering, Zur Lehre vom Lichtsinne, 1878.
J. von Kries, Die Gesichtsempfindungen u. ihre Analyse, 1882.
G. E. Müller, Zeits. f. Psychol., x., 1896, 1, 321.
E. C. Sanford, Course, exps. 113 ff., 122 ff., 135 ff.
W. Wundt, Philos. Studien, iv., 1888, 311.

Cf. also C. L. Franklin, Mind, N. S., ii., 1893, 473; M. Foster, A Textbook of Physiology, iv., 1891, 1222; G. F. Stout, A Manual of Psychology, 1899, 141; Titchener, An Outline of Psych., 1899, 52; O. Külpe, Outlines of Psych., 1895, 112; Wundt, Phys. Psych., i., 1893, 482.

On the colour pyramid see:

Ebbinghaus, 184; Titchener, Primer of Psychol., 1899, 41; K. Zindler, Zeits. f. Psych., xx., 1899, 225; Wundt, Phys. Psych., i., 1893, 504.

It is important that the student thoroughly understand the colour pyramid, and that he shall think of it always as a purely psychological (not physical or physiological) construction.

Question (1) The surface of the figure contains the relatively most saturated colour-tones. Round the base we have the 'pure' colours, red, etc.; towards white, we have the pinks, straw yellows, sky blues, pale greens; towards black, the Bordeaux reds, chocolate browns, indigo blues, dark greens. All these tones are the most saturated possible, the most coloured colours of their kind. Each of them lies upon the straight line which connects their parent spectral colour with white or black, and at the height of the grey (black-white series) to which their brightness corresponds.

If we peel the figure, leaving the black and white poles untouched, we get precisely what we had before, save that all the colour-tones are less saturated, lie so much nearer to the neutral

tones of the axis. It is clear, further, that if we pare the figure with a knife that cuts parallel to the axis and at a definite distance from it, we have upon the cut surface colour-tones which are all of the same degree of saturation.

The cross-section shows us all the colours that exist, which are of the same brightness as the grey of the plane of section.

The longitudinal section shows us (*a*) the neutral axis, and (*b*) all possible nuances of two colour-tones, the tones of the points of section upon the base.

(2) The first part of this question is best answered after the performance of the Preliminary Exercise.

Y is the colour that stands nearest to white, in introspection. As we have assumed that our base lies in a single plane, the complementary *B* must sink correspondingly towards black. *B* is undoubtedly the colour that stands nearest to black; but how far down it should be placed we do not know.

(3) The length of *RY*, etc., depends upon the number of colour-tones that can be distinguished, under similar conditions, between the end-points of the lines. The angles are determined by the number of just noticeable differences of sensation that separate the opposite points *RG* and *YB*, when we pass from point to point, not through saturated colours, but by the shortest possible road, *i.e.*, through the black-white axis. As neither of these determinations has been accurately made, the distances in the Figure are conventional, and the angles have been rounded.

(4) See Hering, Lichtsinn, 89; and p. 44 below.

(5) This question need not be answered at once, but may be assigned as an additional exercise later in the course. The fullest and most impartial account that we have at second hand is that given by Ebbinghaus.

(6) Ebbinghaus, 183, 187, 247, 253. It is important that the physical, physiological and psychological uses of the terms be distinguished.

(7) We have (*a*) the problems of indirect vision and of colour blindness, and (*b*) the problems of the relation of sensation to stimulus. These are five in number: (i) the dependence of sensation upon the wave-length of the stimulus; (ii) its dependence upon the intensity of stimulus: the Purkinje phenomenon;

(iii) its dependence upon the composition of stimulus: colour mixture; (iv) its dependence upon the spatial distribution of stimulus: contrast; and (v) its dependence upon the temporal relations of stimulus: adaptation and after-images.

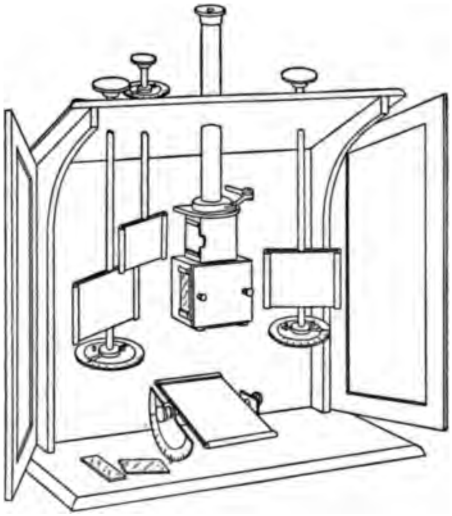


FIG. 1. — Hering's apparatus for testing colour blindness. R. Rothe, Mk. 100. See E. Hering, Arch. f. Ophthalm., xxxvi., 1, 1890, 217.

See Ebbinghaus, 200. We deal with (*b*) (i) in the following preliminary exercise; and with the first part of (*a*), and with (*b*) (iii), (iv), (v), in special Experiments. On the Purkinje phenomenon, see Ebbinghaus, 203. It can be demonstrated as follows. Lay a stick of red sealing wax upon an ultramarine-blue ground that in daylight is distinctly darker than the red. Decrease the illumination. Presently the red becomes a dead black, while the dark blue shows a bright bluish-grey shimmer. *Cf.* E. C. Sanford, A Course in Experimental Psychology, 1898, 142. On colour blindness, see Ebbinghaus, 194, and the bibliography in Helmholtz, Phys. Optik, 1179. For testing, use the Hering apparatus, Fig. 1.

PRELIMINARY EXERCISE. — The spectrum chart may be purchased of the Prang Educational Co., Boston, Mass., for $1.00.

The following points may be noticed. (1) The spectral band represents a large number of sensation qualities, each of which passes over into its neighbour through intermediate qualities. (2) The change of quality is sharply emphasised at certain places in the series. Thus we have continuity from Red to Yellow. Then we seem, as it were, to turn a corner, and travel continuously from Yellow to Green. Again we turn a corner, and get from Green to Blue. Here we turn, for the last time, and can pass without break from Blue, through Violet, back to our starting-point, Red. (3) This last fact brings out another: the fact, namely, that the spectrum has at its extreme ends the

two colours which are most nearly alike. Red is more like Violet than it is even like Orange. (4) And with this goes the further fact, that the spectrum does not show us the full total of colour qualities. By mixing Violet with Red we obtain a series of intermediate Purples. If we think of the Red of the spectrum as prolonged, on our left, through purplish red to Purple, and of the Violet as prolonged, on our right, through violet-purple to Purple, we have a series beginning and ending with the same colour, which does represent the complete tale of colour qualities (see the base of the colour pyramid). (5) The spectrum, again, shows us none of the pure brightness qualities; none of the blacks, whites and greys. (6) It does show us its colours intermixed with different brightnesses. Thus, Yellow is by far the brightest colour of the spectrum ; Violet is the darkest. It is a good exercise to arrange the spectral colours in the order of brightness, from light to dark, within these limits. (7) There is a psychological unfairness, so to speak, in the spatial distribution of the spectral qualities. Different sensations are crowded together, *e.g.*, in the yellow-green region, while there are great bands of red and violet that look almost the same throughout. (8) Probably, in staring at the chart, certain after-image phenomena will arise and attract the observers' attention. (9) The varying degrees of saturation of the qualities may be remarked. Not only is yellow the brightest colour; it is the least coloured colour, the colour most nearly approximating to a pure brightness. The red will, probably, seem to be the most saturated colour of the series; after it come the blue, green and violet. Violet, *i.e.*, though very dark, is well saturated. (10) If the chart has been observed with steady fixation (Hering's local adaptation), a phenomenon will be observed which is the direct opposite of contrast: each patch of colour will seem to spread itself out over the neighbouring portions of the colour series (Hering's simultaneous light induction). The result is, that brightnesses tend all towards a neutral grey, and colours all towards diminished saturation, and hence, ultimately, also towards neutral grey.

It should be impressed, again, upon the student that the 'psychological' spectrum is quite a different matter from the

'physical' spectrum. The purple-extensions of the two ends of the spectrum have, of course, nothing at all to do with the infra-red and the ultra-violet of the physicist. We make use of the spectrum simply because it is a well-known and easily procurable band of colours, which presents all the colour qualities (with the exception of the purples) at their highest saturation: we are not concerned with its physical significance.

EXPERIMENT I

§ 9. **Colour Mixing.** — It will probably be found advantageous, in these experiments, to let each student combine the functions of *O* and *E*, — to let each manage his own mixer, and take his own introspective records. Much time is saved by such an arrangement; and there is no need of any questioning of *O* by *E*, since the verdict of introspection is read off directly from the discs. For quantitative work, it would be better to entrust the changing of the discs and starting of the mixer to *E*, and to direct *O* to turn his eyes upon some indifferently tinted surface, of the average brightness of his surroundings, during the intervals between experiment and experiment. As it is, the student should be instructed to look at the discs for as short a time as possible, compatibly with accurate matching; and to look off towards the grey screen, after the adjustment of discs for a new experiment, in order to satisfy himself that he has no coloured after-image. If such an image appears, he must wait till it has passed away, before making his determination.

It should be noted that mixture experiments with coloured papers are not, as a rule, 'pure' experiments. Yellow and blue, when mixed, give grey. But the standard yellow of a coloured paper series generally contains a certain amount of orange and red; and the standard blue generally contains a certain amount of green and violet. Hence, in mixing 'yellow' and 'blue,' we are really mixing all the colours of the spectrum; our grey is, like daylight, the result of a general mixture. The mixed nature of the coloured-paper colours can be seen by pasting small squares of the papers upon pieces of black card, and looking at them through a prism. Not a single colour is seen, but a fringe of colours. — Nevertheless, the results obtained from mixtures of

these colours are just the same as those which would be obtained from mixtures of pure colours of the same appearance; so that there is nothing in the experiments to mislead the student, when once the fact of impurity has been explained to him.

It should be noted, further, that different authors and different sciences use colour names in different meanings. The 'pure' red of psychological optics, Hering's *Urroth*, is (as we have remarked above) a carmine, a red with a distinctly purplish tinge; not a vermilion, an orange-red. The 'pure' green, Hering's *Urgrün*, is a bluish green, the complementary of carmine. In the list of complementaries in the text, the usual names applied to the spectral colours and their mixtures are employed. The reason is, again, merely one of convenience. These are the colour names in ordinary use, and the coloured papers most readily procurable aim rather at reproducing spectral colours than at showing the psychological primaries.

(1) *First Law.*—The following equations were obtained, in diffuse daylight, by mixture of the Wundt papers (supplied by E. Zimmermann). The series consists of R, O^1 (light), O^2 (dark), Y, YG (very light), G, GB, BG, B^1 (light), B^2 (dark), V and P (reddish).

1. Carmine and bluish green. (Carmine = R + B^1; bluish green = B^1 + G.)

$$125° R + 79° B^1 + 156° G = 92° W + 268° Bk.$$

2. Red and verdigris. (Verdigris = G + BG.)

$$108° R + 40° G + 212° BG = 82° W + 278° Bk.$$

3. Orange and greenish blue. (Greenish blue = G + GB.)

$$113° O^1 + 17° G + 230° GB = 123° W + 237° Bk.$$

$$108° O^2 + 83° G + 169° GB = 97° W + 263° Bk.$$

4. Yellow and blue.

$$162° Y + 198° B^1 = 180° W + 180° Bk.$$

5. Yellowish green and violet. (Yellowish green = G + Y or YG + R.)

(1) $266° V + 40° G + 54° Y = 73° W + 287° Bk.$

(2) $177° V + 134° YG + 49° R = 111° W + 249° Bk.$

6. Green and purple. (Purple = P + B^2.)

$$234° P + 42° B^2 + 84° G = 33° W + 327° Bk.$$

(2) *Second Law.* — The 'intermediate' colours, resulting from the mixture of colours that are not complementary, are just as much sensation qualities, *i.e.*, just as little analysable in introspection, as are the original colours of the mixture. The student is apt to think that the result of mixture in this case is a qualitative perception, and not a sensation quality, — partly because he knows that two colours go to produce it, and partly because he has in many cases only a double name (yellowish green) or a descriptive name (orange) for it. He should, therefore, be told that, if the real spectrum were worked through, line by line, for purposes of discrimination, every discriminable line would represent a sensation, qualitatively different from all the other lines distinguished before or after it. Orange is no more a mixture of red and yellow, in introspection, than white is a mixture of all the spectral colours.

The following equations were obtained, with the Wundt papers, in diffuse daylight.

(1) Y+G=YG.	*Large:*	235° G + 15° Y + 110° W =	*Small:*	360° YG.
(2) Y+R=O.	"	75° Y + 285° R =	"	297° O^2 + 21° W + 42° Bk.
(3) B+G=BG.	"	152° B^2 + 208° G =	"	11° W + 349° BG.
" "	"	137° B^1 + 223° G =	"	8° W + 352° BG.
(4) G+V=B.	"	41° G + 319° V =	"	133° B^2 + 17° W + 210° Bk (dark blue).
(5) R+B=V.	"	37° R + 160° B^2 + 163° Bk =	"	360° V.
(6) R+B=P.	"	13° B^2 + 93° R + 254° Bk =	"	360° P.
" "	"	9° B^1 + 96° R + 255° Bk =	"	360° P.
(7) P+B=V.	"	124° B^1 + 236° P =	"	349° V + 11° W.
" "	"	130° B^2 + 105° P + 125° Bk =	"	360° V.

(3) *Third Law.* — The reason for the condition of constant illumination is given with the occurrence of the Purkinje phenomenon: see p. 7 above, and Ebbinghaus, 214. The law is valid over a fairly wide range of moderate intensities.

The following results were obtained with the Wundt papers, in diffuse daylight.

$$(a)\quad \left.\begin{matrix}\text{Verdigris}\left\{\begin{matrix}\text{BG} & 212^\circ\\ \text{G} & 40^\circ\end{matrix}\right.\\ \text{Red} \ldots\ldots\ 108^\circ\end{matrix}\right\} = \begin{matrix}\text{W} & 82^\circ\\ \text{Bk} & 278^\circ\end{matrix} = \begin{matrix}162^\circ\ \text{GB}\\ 85^\circ\ \text{G}\\ 98^\circ\ \text{O}^2\\ 15^\circ\ \text{Bk}\end{matrix}\ \begin{matrix}\left.\right\}\text{Greenish blue.}\\ \ldots\ \text{Orange.}\\ \\ \end{matrix}$$

Then:

$$\left.\begin{array}{lr} \text{Verdigris} \begin{cases} \text{BG} \\ \text{G} \end{cases} & \begin{array}{r} 106^\circ \\ 20^\circ \end{array} \\ \text{Red} \ldots\ldots & 54^\circ \\ \text{Greenish blue} \begin{cases} \text{GB} \\ \text{G} \end{cases} & \begin{array}{r} 81^\circ \\ 42.5^\circ \end{array} \\ \text{Orange} \ldots \ \text{O}^2 & 49^\circ \\ \text{Black} \ldots\ldots & 7.5^\circ \end{array}\right\} = \text{either the first or the third term of the above equation.}$$

(*b*)

$$\left.\begin{array}{l} \text{Purple} \begin{cases} 244^\circ \text{P} \\ 32^\circ \text{B}^2 \end{cases} \\ \text{Green} \ldots 84^\circ \end{array}\right\} = 40^\circ \text{W} + 320^\circ \text{Bk.}$$

$$\left.\begin{array}{l} \text{Yellowish green } 165^\circ \\ \text{Violet} \begin{cases} 115^\circ \text{V} \\ 80^\circ \text{R} \end{cases} \end{array}\right\} = 144^\circ \text{W} + 216^\circ \text{Bk.}$$

Then: $(122^\circ \text{P} + 16^\circ \text{B}^2) + 42^\circ \text{G} + 82.5 \text{YG} + (57.5 \text{V} + 40^\circ \text{R}) = 92^\circ \text{W} + 268^\circ \text{Bk.}$

(*c*)

$$\left.\begin{array}{l} \text{R } 247^\circ \\ \text{G } 113^\circ \end{array}\right\} = \begin{cases} \text{O}^2 & 156^\circ \\ \text{Bk} & 174^\circ \\ \text{W} & 30^\circ \end{cases}$$

Either is matched by: $185.25 \text{R} + 84.75 \text{G} + 39^\circ \text{O}^2 + 43.5 \text{Bk} + 7.5^\circ \text{W}$.

COROLLARIES.—(1) Three colours, properly chosen and proportioned, will give grey when mixed. The conditions are fulfilled when the three can be split up into four, which represent two pairs of complementaries. Thus R, G and V can be split up into R, YG, BG, V (second law). Here are two pairs of complementaries; grey results from a rightly proportioned mixture (first law).

So with R, Y and GB. These colours are equivalent to R, Y, BG, B (second law). The Y and the B cancel; the R cancels the BG (verdigris). Grey results (first law).

The following results were obtained with the Wundt papers, in diffuse daylight.

$$80^\circ \text{R} + 123^\circ \text{G} + 157^\circ \text{V} = 65^\circ \text{W} + 295^\circ \text{Bk.}$$

$$74^\circ \text{R} + 72^\circ \text{Y} + 214^\circ \text{GB} = 119^\circ \text{W} + 241^\circ \text{Bk.}$$

Let the student work out similar sets of three for himself, from the spectral colours and purple.

(2) These sets of three, rightly proportioned, will give any colour, as well as grey; the whole spectral series, rightly mixed, will give any colour, or grey; all the spectral colours, with purple, will give any colour, or grey.

Take, *e.g.*, R, G, V. These are equal to R, YG, BG, V. Adjust them to give a grey. Now (*a*) make up a disc of 270° of this grey mixture, and fill in the remaining 90° with R. You get an R. (*b*) Repeat, but fill in the 90° with G. You get a G. (*c*) Repeat, with the 90° of V. You get a V. (*d*) Split up the colours still further. YG is equal to Y and G. G is equal to YG and BG. You now have, then, R, Y, YG, BG, BG again, V. If the R cancels the two BG, and the V the YG, the Y must emerge.

(*e*) Get B in the same way. Split up the four colours into R, YG, B, G, V. This is equal to R, YG, B, YG, BG, V. With enough R to cancel the BG, and enough V to cancel the two YG, B emerges. Illustration:

$$80^\circ \text{R} + 123^\circ \text{G} + 157^\circ \text{V} = 65^\circ \text{W} + 295^\circ \text{Bk}.$$

(*a*) $(60^\circ \text{R} + 92.25^\circ \text{G} + 117.75^\circ \text{V}) + 90^\circ \text{R} = (48.75^\circ \text{W} + 221.25^\circ \text{Bk}) + 90^\circ \text{R}.$

(*b*) $(60^\circ \text{R} + 92.25^\circ \text{G} + 117.75^\circ \text{V}) + 90^\circ \text{G} = (48.75^\circ \text{W} + 221.25^\circ \text{Bk}) + 90^\circ \text{G}.$

(*c*) $(60^\circ \text{R} + 92.25^\circ \text{G} + 117.75^\circ \text{V}) + 90^\circ \text{V} = (48.75^\circ \text{W} + 221.25^\circ \text{Bk}) + 90^\circ \text{V}.$

(*d*) $161^\circ \text{R} + 193^\circ \text{G} + 6^\circ \text{V} = 35^\circ \text{W} + 241^\circ \text{Bk} + 84^\circ \text{Y}.$

(*e*) $30^\circ \text{R} + 85^\circ \text{G} + 245^\circ \text{V} = 39^\circ \text{W} + 261^\circ \text{Bk} + 60^\circ \text{B}.$

Let the student work out other examples for himself.

(3) A third corollary, which may be regarded as the obverse of the second, is as follows: any unsaturated colour may be matched by the mixture of the corresponding saturated colour and white. For white — a white of general validity — may be produced, by the third law, through the mixture of saturated complementaries in the right proportions. Let the colour in question be supposed to be one of these complementaries. If it be present in such quantity that white cannot result from its mixture with the complementary, then the result of the mixture (by the first law) is an unsaturated colour of the prevailing tone. But this is the result which the corollary calls for.

The student has already matched complementary greys with black-white greys. Let him now take two discs, composed of 270° of two such matched greys and 90° of one or other of the colours employed to produce the complementary grey. The two mixtures will match.

QUESTIONS. — (1) The characteristics are: (i) purity, *i.e.*, that the red be not a red that is half orange, the green not a yellowish green, etc.; (ii) a high degree of saturation of all colours; (iii) as nearly an equality of brightness throughout the series as is compatible with good saturation of the colours; (iv) a dead, dull surface, — no shininess or glazing; (v) a closely-woven, stringy or parchment-like texture, — so that the papers do not fray at the edges, or wear away at the centre (the latter fault is common to very many of the papers on the market, and is extremely annoying); (vi) full representation of the scale of colour qualities, and a selection of qualities on a psychological basis, — so that the step from colour to colour is a psychologically, not a physically, equal (or roughly equal) step in each case. — See above, p. 10.

(2) Contrast (see Exp. III.). If, *e.g.*, the large discs were too red, the margin of the smaller discs would look greenish.

(3) See above, p. 12.

(4) There are various ways. (i) Superposition of parts of two spectra. This is the best and only entirely reliable method. (ii) Irradiation: juxtaposition of small, variously coloured surfaces. This is used in oil painting, tapestry weaving, etc.; it is of little value in the laboratory, although the following short experiment may be given. Cut narrow strips, of equal width, from two coloured papers. Weave them together into a square, like a kindergarten mat. Set them up behind a window cut in a neutral grey cardboard, and walk away from this until the lines disappear, and the colour surface is uniform. Compare its impression with the impression made by a compound disc (180° of each colour) rotating before a similar background at the same distance. — The experiment will familiarise the student with the meaning of 'irradiation,' 'dispersion circles,' etc. (iii) Reflection. Lay two 5 cm. squares of coloured paper, 14 cm. apart, on a background of black velvet or cardboard, 50 by 25 cm. Seat yourself at a table, with the narrow edge of this background close in towards your chest. Lay your elbows on the table, and hold up, midway between the paper squares, a piece of clear glass 30 cm. high and 25 cm. broad. Lean your head down, and incline the glass towards you, so that your forehead may rest

upon the edge of the glass. You now see the further square by transmitted light and the nearer square by reflected light. If the squares are rightly placed, their colours overlap and a true mixture results.—To get the colours at equal intensities, you may move the squares nearer to or farther from the glass, or tilt the glass itself to different angles. The nearer the squares to the glass, and the less nearly the angle of glass with background forms a right angle, the more intensive is the reflected colour as compared with the transmitted.—Mix two coloured papers in this way, and then assure yourself, by comparison, that the result of their mixture is the same with that obtained from rotation of the same papers. This method is intrinsically good, but lacks many of the advantages of the method of rotation. It was devised by J. H. Lambert: Photometria, sive de mensura et gradibus luminis, colorum et umbrae, 1760, p. 527. (iv) Double refraction. Look through an achromatic, doubly refracting prism of quartz or Iceland spar, at two coloured squares laid side by side. The fields of colour will partially overlap; and where they do so, there is mixture as there is in Lambert's experiment. Only, the brightness of each of the overlapped portions is considerably reduced.—This method has been used by Hering to demonstrate the third law of mixture. (v) We may use the method of rotation with transmitted, in place of reflected light. Cut 'skeleton' discs of a not too heavy black card,—discs, *i.e.*, 20 cm. in diameter, having a rim 1 cm. wide, a solid centre piece of 6 cm. diameter, and four sectors of 10° each; all the rest is cut away. Between two of these skeleton discs lay discs cut from sheets of transparent coloured gelatine. Mount on the mixer, as for the ordinary paper discs. Place *O* in the dark room, and show him the colours by transmitted (day or artificial) light. The method has the advantages that the gelatine mixtures can be tested directly with the spectroscope, and their spectral values thus determined, and that certain mixtures give pure spectral colours. It has the disadvantages of being more tedious even than the regular method, with reflected light; of requiring more elaborate arrangements; and of presenting greater difficulties to judgment, *e.g.*, in the case of complementarism.

It is not likely that any student will recommend mixture of pigments. Should this method be mentioned, however, its radical difference from all the above-named procedures must be fully explained.

The method of mixture by rotating discs appears to originate with P. van Musschenbroek (Introductio ad philosophiam, 1768, ii., § 1820). It was employed later by J. Plateau (Poggendorff's Annalen, lxxxviii., 1853, p. 172), and perfected by J. Clerk Maxwell (Trans. R. S. Edin., xxi., 1857, p. 275). Its advantages are: that the magnitude of any given sector can be readily changed and accurately measured, and that brightness matches and colour equations of all kinds can be easily obtained and verified.

ADDITIONAL QUESTIONS.—(1) See Helmholtz, Physiol. Optik, 2d ed., pp. 311 ff.; Hering, Zur Lehre vom Lichtsinne, v., vi., 1878 (reprint of work published 1872–4); Ueber Newton's Gesetz der Farbenmischung, 1887 (off-print from Lotos, N. F., vii.: often found bound up with the foregoing); Eine Vorrichtung zur Farbenmischung, etc., Pflüger's Archiv, xlii., 1888, pp. 119 ff. See also A. Tschermak, Ueber die Bedeutung der Lichtstärke und des Zustandes des Sehorganes für farblose optische Gleichungen, Pflüger's Archiv, lxx., 1898, pp. 297 ff. —A good popular account of the two theories is given by Ebbinghaus, Psychologie, i., pp. 209–217, 245–263.

(2) Hering, Zur Lehre vom Lichtsinne, iii., v.; Ueber die von v. Kries wider die Theorie der Gegenfarben erhobenen Einwände, Pflüger's Archiv, xlii., pp. 488 ff.; xliii., pp. 264 ff., 329 ff.; Ebbinghaus, Psychologie, i., pp. 230 ff.

(3) Sir I. Newton works out the first two laws, in his Opticks, Bk. I., Part ii., prop. iv–vi, Opera, edition of 1782, iv., pp. 85–100. The third law was formulated by H. Grassmann, Zur Theorie der Farbenmischung, Poggendorff's Annalen, lxxxix., 1853, pp. 69–84; Philos. Mag., (4) vii., 1853, pp. 254–264.

INSTRUMENTS.—Helmholtz' Spectrophotometer is described in his Physiol. Optik, 2d ed., pp. 355 ff. For a cut of Hering's

indirect-vision colour mixer, see p. 20 below. K. Marbe's colour mixer (with change of sectors during rotation) is described in the Année Psych., ii., 1896, 752; v., 1899, 391: Zimmermann, Mk. 100 or 140, without motor; Mk. 240, with motor.

EXPERIMENT II

§ 10. **Campimetry.** *Cautions not noted in the Text.* — The distance of the campimeter from the observing eye may be regulated by *O*'s convenience. It must neither be so small as to bring the white fixation-point this side of the limit of clear vision, nor so large that the limits of the zones cannot be marked upon the various meridians.

Care must be taken that *O* can balance himself in an easy and steady attitude. A very slight amount of bodily discomfort will distract the attention in these experiments. If the table is too low, the whole apparatus, colour mixer included, may be raised upon boards, clamped to the edges of the table. — The eye-rest must be accurately adjusted by *E*, so that the centre of the circular opening lies directly over against the 'yellow spot,' or 'spot of clearest vision,' when the eye is looking straight downwards. — The illumination should be so regulated that no shadow falls upon any part of campimeter or stimulus disc.

When black is added to the brighter-looking (or white to the darker-looking) colour, in exp. (3), the proportion of the coloured sectors in any compound disc must, of course, remain unchanged. If we have a disc of 278° R and 82° B, and we wish to add in 20° W, our resultant disc consists of 262.5° R, 77.5° B, and 20° W. The ratio R : B remains unchanged.

If a mechanical colour mixer is employed, three students must be assigned to these experiments: an *O*, an *E* who shall move the fixation-mark, etc., and an *E* who shall rotate the discs. If an electric motor mixer be used, one *E* is sufficient.

Where the mm. scale is not printed on the campimeter, the procedure must be modified a little. *O* must never be allowed to see the white strip. In exp. (1), a pencil line ruled on the cardboard may serve as track for the fixation-point, and a pencil dot may mark the zone boundaries. Or, if the 'step' method be

employed (see below, pp. 18 f.) the .5 cm. or 1 cm. steps may be marked beforehand by pencil dots, and so on.

Since the retina is not a plane surface, but a portion of the inner surface of a hollow sphere, the linear values obtained for the zones on the different meridians should be translated into degrees and minutes. Two distances are known: the distance of the observing eye from the centre of the circular opening which lies directly beneath it, and the distance from this centre to the limit of the zone in question. The ratio of this latter distance to the former is the tangent of the angle enclosed by the line of regard and the line drawn from the centre of the circular opening to the eye. Knowing the tangent, we know the arc subtended by the angle. The value of this arc should replace the linear value in *E*'s Tables. This is, of course, a purely mathematical, not a psychological exercise. If the student is at all pressed for time, he may be excused from performing it. The linear determinations, and the maps draughted from them, give all the necessary psychological facts. If the calculation is made, it is desirable to have the maps drawn upon the printed perimeter charts sold by opticians.

Experiment (1). — Some observers find it more natural and less fatiguing to move the eye down than to move it to the right. In their case the experiment should begin not with the right horizontal meridian (temporal half of retina) but with the lower vertical (lower half of retina). It may be said, in general, that downward movement of the eye is easier than upward, and outward movement easier than inward. The left horizontal (nasal) meridian is the most difficult of all; partly, no doubt, because the blind-spot causes a total disappearance of the opening and colour at a certain point upon the scale (4 to 5 cm., in the experiments quoted below).

Again: for some observers it is almost impossible to find a rate of movement of the fixation-point which shall satisfy the conditions of the experiment and yet not allow of the formation of after-images. Under these circumstances it is better to move the fixation-mark by definite steps (5 mm. or 1 cm.), and to make a separate experiment at each step. *O* stands with his closed

eye settled in the eye-rest; at *E*'s "Now!" he opens the observing eye, and turns it sharply to the fixation-mark. As soon as he has made his observation, — and this should be done almost instantaneously, — he turns his eye back to its original position, and closes it. The observation is recorded; the mark quickly moved out one step farther; and the "Now!" at once repeated. A series of experiments performed by this method will, of course, require more time, however deftly it be carried out, than an experiment performed with continuous movement of the fixation-point.

The following are records of actual experiments. All measurements are made from the centre of the campimeter opening.

Disc: Hering's 'red' paper. Height of eye-rest above screen, 14.3 cm.

LEFT HORIZONTAL MERIDIAN. (Nasal retina.)

Outgoing	mark	(from	1	cm.):	red	sensed	as	yellow	at	8	cm.
"	"	"	1	"	"	"	"	"	"	9	"
Incoming	"	"	11	"	"	"	"	red	"	7	"
"	"	"	11	"	"	"	"	"	"	6	"
Outgoing	"	"	5	"	"	"	"	black	"	16	"
"	"	"	5	"	"	"	"	"	"	14	"
Incoming	"	"	17	"	"	"	"	yellow	"	13	"
"	"	"	17	"	"	"	"	"	"	14	"

RIGHT HORIZONTAL MERIDIAN. (Temporal retina.)

Outgoing	mark	(from	1	cm.):	red	sensed	as	yellow	at	5	cm.
"	"	"	1	"	"	"	"	"	"	6	"
Incoming	"	"	10	"	"	"	"	red	"	4	"
"	"	"	10	"	"	"	"	"	"	3	"
Outgoing	"	"	5	"	"	"	"	black	"	14	"
"	"	"	5	"	"	"	"	"	"	14	"
Incoming	"	"	16	"	"	"	"	yellow	"	13	"
"	"	"	16	"	"	"	"	"	"	13	"

UPPER VERTICAL MERIDIAN. (Upper retina.)

Outgoing	mark	(from	1	cm.):	red	sensed	as	yellow	at	3	cm.
"	"	"	1	"	"	"	"	"	"	3	"
Incoming	"	"	5	"	"	"	"	red	"	2	"
"	"	"	5	"	"	"	"	"	"	2	"
Outgoing	"	"	4	"	"	"	"	black	"	12	"
"	"	"	4	"	"	"	"	"	"	11	"
Incoming	"	"	13	"	"	"	"	yellow	"	11	"
"	"	"	13	"	"	"	"	"	"	12	"

LOWER VERTICAL MERIDIAN. (Lower retina.)

Outgoing	mark	(from	1	cm.) :	red	sensed	as	yellow	at	2	cm.
"	"	"	1	"	"	"	"	"	"	2	"
Incoming	"	"	4	"	"	"	"	red	"	1	"
"	"	"	4	"	"	"	"	"	"	2	"
Outgoing	"	"	3	"	"	"	"	black	"	8	"
"	"	"	3	"	"	"	"	"	"	8	"
Incoming	"	"	10	"	"	"	"	yellow	"	8	"
"	"	"	10	"	"	"	"	"	"	7	"

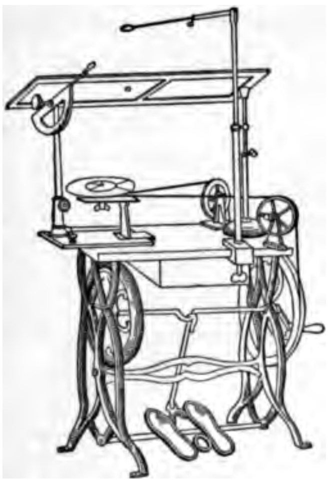


FIG. 2. — The Hering indirect-vision colour mixer. Rothe, Mk. 147. See E. Hering, Arch. f. Ophthalm., xxxv., 4, 1889, 63.

EXPERIMENT (2). — The psychological red for the observer *A. B.* was determined as Red 306° + Blue 54° (Hering's papers). Right eye employed.

(1) *Date*: 7/5/99. 3 P.M.[1] Uniformly cloudy sky.

E: X. Y.

O: A. B. Condition normal.

Materials: Hering indirect-vision colour mixer. Red and blue discs (Hering's papers). Grey screen[2] no. 2. White fixation-mark on black straw. Mm. paper scale.

Method: Movement of fixation-point by 1 cm. steps. Measurements taken from centre of stimulus circle (centre of circle stamped from grey screen). Height of eye-rest above screen 14.3 cm.

Results: LEFT HORIZONTAL MERIDIAN. (Nasal retina.)

Outgoing	mark	(from	1	cm.) :	red	sensed	as	black	at	15	cm.	(44° 43')[3]
Incoming	"	"	20	"	"	"	"	red	"	14	"	(42° 50')
Outgoing	"	"	1	"	"	"	"	black	"	15	"	(44° 43')
Incoming	"	"	20	"	"	"	"	red	"	15	"	(44° 43')

[1] Good results can be obtained, at this season of the year, in the early morning: say, from 5 to 7.30 A.M.

[2] Four screens are given with the Hering mixer: no. 1, light grey; no. 2, neutral grey; no. 3, dark grey; no. 4, very dark grey.

[3] In the calculation of these angular values, the distance of the campimeter surface from the nodal point of the observing eye was taken as 15.1 cm. From campimeter to under surface of eye-rest was 14.3 cm.; from corneal surface to nodal point is ap-

(2) *Date:* 7/5/99. Conditions as before.

RIGHT HORIZONTAL MERIDIAN. (Temporal retina.)

Outgoing	mark	(from 1 cm.) :	red	sensed	as grey	at	9 cm.	(30° 48′)
Incoming	"	" 15 "	"	"	" red	"	9 "	(30° 48′)
Outgoing	"	" 1 "	"	"	" grey	"	9 "	(30° 48′)
Incoming	"	" 15 "	"	"	" red	"	8 "	(27° 53′)

(3) *Date:* 7/5/99. Conditions as before.

UPPER VERTICAL MERIDIAN. (Upper retina.)

Outgoing	mark	(from 1 cm.) :	red	sensed	as black	at	8 cm.	(27° 53′)
Incoming	"	" 11 "	"	"	" red	"	7 "	(24° 51′)
Outgoing	"	" 1 "	"	"	" black	"	8 "	(27° 53′)
Incoming	"	" 11 "	"	"	" red	"	8 "	(27° 53′)

(4) *Date:* 7/5/99. Conditions as before.

LOWER VERTICAL MERIDIAN. (Lower retina.)

Outgoing	mark	(from 1 cm.) :	red	sensed	as black	at	6 cm.	(21° 40′)
Incoming	"	" 12 "	"	"	" red	"	7 "	(24° 51′)
Outgoing	"	" 1 "	"	"	" black	"	6 "	(21° 40′)
Incoming	"	" 12 "	"	"	" red	"	7 "	(24° 51′)

The psychological green for the same *O* was determined as Green 230° + Blue 130° (Hering's papers). Right eye.

(1) *Date:* 11/5/99. 3 P.M. Cloudy sky.
E: X. Y. *O:* A. B. Condition normal.
Materials: As before. Green and blue discs (Hering's papers).
Method: As before. Height of eye-rest above screen 14.3 cm.

Results: LEFT HORIZONTAL MERIDIAN. (Nasal retina.)

Outgoing	mark	(from 1 cm.) :	green	sensed	as whitish grey	at	16 cm.	(46° 24′)
Incoming	"	" 20 "	"	"	" green	"	15 "	(44° 43′)
Outgoing	"	" 1 "	"	"	" whitish grey	"	16 "	(46° 24′)
Incoming	"	" 20 "	"	"	" green	"	14 "	(42° 50′)

proximately 6 mm.; and the corneal surface lay, by rough measurement, about 2 mm. above the under surface of the eye-rest. — The blind spot falls, as we saw just now, at 4 to 5 cm. from the centre of the campimeter opening. This gives an angle of 18° 20′ to 14° 48′; values which square well with those observed by Helmholtz for the width of the spot, — 18° 55′ to 12° 25′ (Physiol. Optik, 2d ed., p. 253).

(2) *Date:* 11/5/99. Conditions as before.

RIGHT HORIZONTAL MERIDIAN. (Temporal retina.)

Outgoing mark	(from	1 cm.):	green	sensed	as white	at 8	cm.	(27° 53′)
Incoming	“	“ 18 “	“	“	“ green	“ 7	“	(24° 51′)
Outgoing	“	“ 1 “	“	“	“ white	“ 8	“	(27° 53′)
Incoming	“	“ 18 “	“	“	“ green	“ 7	“	(24° 51′)

(3) *Date:* 11/5/99. Conditions as before.

UPPER VERTICAL MERIDIAN. (Upper retina.)

Outgoing mark	(from	1 cm.):	green	sensed	as white	at 9	cm.	(30° 48′)
Incoming	“	“ 11 “	“	“	“ green	“ 7	“	(24° 51′)
Outgoing	“	“ 1 “	“	“	“ white	“ 8	“	(27° 53′)
Incoming	“	“ 11 “	“	“	“ green	“ 7	“	(24° 51′)

(4) *Date:* 11/5/99. Conditions as before.

LOWER VERTICAL MERIDIAN. (Lower retina.)

Outgoing mark	(from	1 cm.):	green	sensed	as white	at 7	cm.	(24° 51′)
Incoming	“	“ 11 “	“	“	“ green	“ 6	“	(21° 40′)
Outgoing	“	“ 1 “	“	“	“ white	“ 6	“	(21° 40′)
Incoming	“	“ 11 “	“	“	“ green	“ 5	“	(18° 20′)

EXPERIMENT (3). — The brightness-equation of the primary red and the primary green, for the observer A. B., right eye, was as follows.

(1) *Date:* 27/5/99. 3 P.M. Dull grey sky.
E: X. Y. *O:* A. B. Condition normal.
Materials: Two colour mixers. Red, blue, green, black discs (Hering's papers).
Red 306° + Blue 54° = Green 172° + Blue 98° + Black 90°.

(2) *Conditions* as before.
Materials: Hering's indirect-vision colour mixer, etc.
Method: As in previous experiments. Height of eye-rest, 14.3 cm.

RIGHT HORIZONTAL MERIDIAN. (Temporal retina.)

	Red.			*Green.*		
Outgoing:	sensed black at	9 cm.	(30° 48′)	Sensed black at	8 cm.	(27° 53′)
Incoming:	“ red “	8 “	(27° 53′)	“ green “	8 “	(27° 53′)

(3) LEFT HORIZONTAL MERIDIAN. (Nasal retina.)

Outgoing:	sensed black at	14 cm.	(42° 50′)	Sensed black at	13 cm.	(40° 42′)
Incoming:	“ red “	13 “	(40° 42′)	“ green “	13 “	(40° 42′)

(4) UPPER VERTICAL MERIDIAN. (Upper retina.)

Outgoing:	sensed black at 10 cm. (33° 31′)	Sensed black at 10 cm. (33° 31′)
Incoming:	" red " 8 " (27° 53′)	" green " 9 " (30° 48′)

(5) LOWER VERTICAL MERIDIAN. (Lower retina.)

Outgoing:	sensed black at 7 cm. (24° 51′)	Sensed black at 8 cm. (27° 53′)
Incoming:	" red " 7 " (24° 51′)	" green " 7 " (24° 51′)

An experiment made upon the same *O*, under similar conditions, with blue and yellow, gave the following results.

(1) *Date:* 24/5/99. 9 A.M. Dull sky.
Blue 360° = Yellow 60° + Black 300°.

(2) RIGHT HORIZONTAL MERIDIAN. (Temporal retina.) Eye-rest 14.3 cm.
Yellow: out, 14 cm.; in, 13 cm. Blue: out, 14 cm.; in, 14 cm.

(3) LEFT HORIZONTAL MERIDIAN. (Nasal retina.)
Yellow: out, 11 cm.; in, 11 cm. Blue: out, 12 cm.; in, 10 cm.

(4) LOWER VERTICAL MERIDIAN. (Lower retina.)
Yellow: out, 9 cm.; in, 8 cm. Blue: out, 9 cm.; in, 8 cm.

(5) UPPER VERTICAL MERIDIAN. (Upper retina.)
The limit of the colour-zone in this experiment lay beyond the limits of the campimeter.[1] Approximate values were:
Yellow: in and out, 13 cm. Blue: in and out, 13 cm.

Equations employed on other occasions were:

Yellow 254° + Black 106° = Blue 255° + White 105°;
" 185° + " 175° = " 290° + " 70°.

It will be noticed that the blue and yellow of the Hering papers gave, in these experiments, the primary psychological blue and yellow, whereas the red and green of the papers both demanded an intermixture of blue. The 'black' of the experiments is the velvet-black, not the black tissue-paper, of the Hering series.

QUESTIONS. — (1) The reason is, that the extent of the zones, though constant for a stimulus of given extent and intensity,

[1] The Hering screens are 69.6 cm. long by 23.3 cm. wide. The circular opening (1.4 cm. in diameter) is so placed that the longitudinal distances on either side are 27.9 and 40.3 cm., and the vertical 11.1 and 10.8 cm., respectively.

shown under constant illumination, varies with variation of the extent or intensity of stimulus. With the form of campimeter employed, the extent of the stimulus is kept constant. But we cannot equalise the brightness-values and saturation-values of the coloured papers. The red disc is a good deal brighter than the disc composed of red and blue (primary red): hence the red zone, as marked out by it, is wider than in the second set of experiments. The matter is further complicated, however, by the variability of the general illumination of the stimuli. The darker the day, the less intensive is the stimulus; and, consequently, the smaller are the colour-zones. Hence the two determinations may approximate, if the red disc is given on a dull day, and the red-blue (primary red) disc on a bright day. The experiments should always be performed in diffuse daylight, and care should be taken to have the conditions of illumination as constant as possible. — *Cf.* exp. (3).

(2) The advantages are two. (*a*) The extent of the coloured stimulus remains constant. If it were moved, it would grow smaller (subtend a smaller angle on the retina) as it travelled out, and grow larger (subtend a wider angle) as it came inwards. But, as we have seen, the extent of the zones varies with varying extent of stimulus. If a red stimulus were employed, then, the boundary of the blue-yellow zone, the point at which the red turned to yellow, would be brought nearer the boundary of the innermost efficient zone than it should be: the stimulus at the border of the first and second (efficient and blue-yellow) zones would be more extensive than at the border of the second and third (blue-yellow and black-white) zones. (*b*) If we keep the coloured stimulus stationary, we can use the colour mixer, and so obtain the *Urfarben*. If the stimulus discs could not be rotated, we should be obliged to content ourselves with single pieces of coloured paper: and it would be only by the merest chance that we found a coloured paper which gave an *Urfarbe*. A campimeter could, certainly, be made, which should allow of movement of the colour mixer along its meridians; but it would be a costly and elaborate instrument.

The disadvantage is, that *O*'s eye soon becomes fatigued by the unusually extensive movements which it is called upon to

make. The experiment consequently requires a considerable amount of time for its performance.

(3) No. Objects seen in indirect vision, under the conditions of the rough test proposed, are seen in their normal colours.

The explanation of this fact belongs to the psychology of perception, not to that of sensation. It may be stated as follows. — All the objects to which we turn our attention in indirect vision (to which we attend though we are looking elsewhere) are localised, *i.e.*, are seen as situated at different places, lying in different positions. The group of sensations composing an 'object' has attached to it a 'local mark' or 'local sign.' The local sign is, in all probability, derived from two sources. (*a*) The eye is a moving organ, and turns naturally to the object of attention. If it is held fixed, there is still a *tendency* to turn it towards the object of attention. The tendency, if it were itself made the subject-matter of introspection, would reveal itself as a memory-image of the organic sensations set up round about the eyeball by actual movement, by the actual turn of the eye towards the object of attention. The organic sensations evidently differ in extent and intensity, according to the locality of the object towards which the eye is turned; and their memory-images differ in a similar manner. (*b*) The retina, as this experiment has shown us, yields different colour sensations according as the coloured object mirrors itself upon the innermost, middle, or outer retinal zone. — These two things, the special memory-image of a special movement and the special colour-tint of a special zone, apparently fuse or weld together to constitute the local sign in each particular case. Neither memory-image nor colour-tint is experienced singly, as itself; the fusion of the two is experienced as a conscious 'thereness.' We do *not* note the colour differences, then, in the sense that we do not see objects in zones of colour corresponding to the retinal zones: we *do* note them, if the theory here outlined is correct, in the sense that we perceive the objects about us as placed in different positions. The colour differences are lost to the psychology of sensation: we can remark them as such only under artificial, experimental conditions: they reappear, in another guise, in the psychology of visual space perception. This

is one of the chief problems of experimental psychology, — to unearth the elementary processes buried in perception and idea; to identify them, in spite of all the changes of function that they evince when connected with other elementary processes to form a single complex.

(4) The arrangement suggests that black-white-grey vision is the earliest form of visual sensation; that blue and yellow were next developed; and that red and green are the latest colours that vision has acquired.

We know that ordinary partial colour-blindness is red-green blindness: red and green are the first colours to be lost. This is natural, if they are the last to be gained. Again: a number of cases of total colour-blindness have been described; cases in which nothing is seen but the black-white-grey series. Again: if one is blind to black-white-grey, one is wholly blind: no cases exist of black-white-grey blindness with retention of colour vision. All this is direct evidence in favour of the theory suggested, and there is much indirect evidence besides.

On the other hand, the theory would require modification if it could be proved that certain people are blue-yellow, but not red-green blind. Statements to this effect are sometimes made; but no demonstrative proof has been brought forward. We must therefore suspend our judgments, until further investigations have been carried out. In the meantime, however, we have the full right to say that the trend of all valid testimony, so far, is in favour of our theory.

LITERATURE. — Ebbinghaus, Psych., i., 191; Helmholtz, Physiol. Optik, 2d ed., 372–374; E. Hering, Ueber die Hypothesen für Erklärung der peripheren Farbenblindheit, Arch. f. Ophthalmologie, xxxv., 4, 1889, 63 ff.; xxxvi., 1, 1890, 264; C. Hess, Ueber den Farbensinn bei indirektem Sehen, Arch. f. Ophthalmologie, xxxv., 4, 1889, 1 ff.; A. Kirschmann, Philos. Studien, viii., 1893, 592; Wundt, Phys. Psych., i., 505.

INSTRUMENTS. — Wundt's large perimeter for rotating discs (made by Zimmermann, Mk. 350) is figured in the Philos. Studien, xv., 1900, 526.

§ 11. **Related Experiments.** — (1) Since our work upon the nasal horizontal meridian has reminded us of the existence of

the blind spot, it may be worth while to make it the subject of experiment.

(*a*) *Mapping the Blind Spot.* — *O* sits at a distance of about 2 m. from a wall or screen, his chin supported in a head-rest, and his right eye closely (but not too tightly) bandaged. On the wall is spread a sheet of heavy white paper, 1 m. by 55 cm. In the right hand upper corner, 20 cm. from the upper and 10 cm. from the outer edge of the sheet, is drawn a heavy black cross, with 3 cm. arms. This is the fixation-mark.

On the left hand part of the sheet a point is lightly marked in pencil, 27.5 cm. from the upper, and 30 cm. from the outer edge. Through this as centre are ruled four diameters of a circle of 25 cm. radius: vertical, horizontal, and two oblique.

E has a light rod or bit of stiff card, covered with paper of the same kind as the sheet and carrying at its tip a disc of black paper, 2 cm. in diameter.

For the experiment, *O* sits with his open eye directly opposite the fixation-mark. *E* slowly passes the black disc along one of the ruled lines (direction indifferent). *O* calls out at the moment that the black disc enters the blind spot (disappears), and again at the moment that it emerges from the spot (reappears). *E* makes light pencil marks upon the sheet at these points; it is best to put a figure, showing the number of the test, and a small arrow, showing the direction of movement.

This procedure is repeated until each of the meridians has been worked over twice (once in each direction). The two marks (ingoing and outcoming) at any given point will not, of course, exactly coincide. If there were nothing else to prevent, there would still be the width of the disc: for *O* does not call out till the disc has wholly disappeared, whereas he will call out as soon as ever a margin of it reappears. The outline of the projection of the spot must, therefore, pass through points lying midway between these two. If, however, two determinations at any point show very considerable divergence, the tests along the meridian in question should be repeated.

The number of meridians may be increased, if increased accuracy of outline be desired. If the student wish, *e.g.*, to trace the great vessels that enter the eye with the nerve, he

must work very carefully over the part of the spot that lies above the fixation-mark, and over the lower portion of its inner (right hand) boundary line.

Figures of the blind spot are given in Helmholtz, Physiol. Optik, 2d ed., 252; Titchener, Outline of Psychology, 1899, 176.

(*b*) *The Filling-out of the Blind Spot.* — In ordinary binocular vision, the area that is blind in the one eye is able to see in the other; so that the existence of the blind spot need not be remarked. In ordinary monocular vision, the eye moves so much as a matter of course, and the blind spot lies so far laterally from the spot of clearest vision (covering the space from about 12° to 18° nasalwards from the centre of the macula lutea), that again its existence need not be remarked. But when we are under experimental conditions, and have mapped the spot, we may go on to ask whether (and if so, with what) the blind spot is filled out.

(i) If the blind spot were not filled out, its edges would come together, and there would be a shrinking of space-values over this area of the retina. Although such a shrinking has in fact been reported by some observers, it seems to be due rather to a 'suggestion' from the blind spot itself than to actual observation. The following tests may be made.[1]

a. Paste on a card nine large letters, as follows:

A B C
D E F
G H I

Set up the card at such a distance that E falls upon the blind spot, while all the other letters are visible. Notice that the letters ABCFIHGD form a square; that there is no bending inwards of the boundary lines of the whole figure.

b. Paste on a card a horizontal line of three wafers, 1.5 cm. in diameter, the middle one blue and the two others red. Set up the card at such a distance that the blue wafer disappears, while the reds are seen. Notice that

[1] White cards, 15 by 10 cm., upon which the letters, circles, etc., can be pasted, are convenient for these experiments. The distance from the centre of the fixation-cross to the near edge of the figure intended to fall upon the blind spot may be taken at about 8.5 cm. — A set of four such cards, designed by E. W. Scripture, is sold for 15¢ by E. G. Willyoung, 82–84 Fulton Street, New York. Unfortunately, the fixation-marks on these cards are printed too low.

the distance between the two reds does not seem to shrink; the wafers do not run together.

c. Paste upon a card 30 cm. long a central fixation-mark, and on either side of it, rather lower down and at a distance of 9 cm., a coloured ring large enough to surround the blind spot. Set up the card at such a distance that the blind spot does as a matter of fact fall entirely within one of the circles. Notice that this circle (nasal retina) does not appear smaller than the other (temporal retina).

These experiments can be varied at will. In *a*, *e.g.*, a single line may be drawn through the blind spot. Is it shortened? Or parallels may pass through it. Do they converge on entering and diverge on leaving the spot? Or a square of small letters may be used, and two of these, on either side of the spot, made the objects of special attention. Are they drawn together? — In *b*, the colours of the wafers may be varied, or their number, size, shape, etc. The student should make a series of these cards for himself.

(ii) The blind spot, then, has the same spatial value as the surrounding portion of the retina. How is it filled out? When it falls upon an uniform ground, it is filled out by the quality of that ground: here all observers agree.

a. Paste on a card a coloured ring, large enough to contain the blind spot within its outer, but so wide as not to allow the spot to fall entirely within its inner circumference. Notice that the whole surface appears in the colour of the ring.

The same thing occurs with only partially uniform surfaces.

b. Substitute for the coloured ring a ring cut from a newspaper. Notice that, at any rate until practice has advanced a considerable distance, the whole surface seems to be filled out with printed letters.

On the other hand, our judgment in the case of surfaces that are widely different seems to be a function of our skill and practice in indirect vision.

c. Cover a card over 9 cm. of its length with a neutral grey paper. Over this lay a rectangular cross, of 9 cm. arms; let the vertical arm be white and the horizontal black. Where the arms cross, they must be mitred; not left square.

Prepare similar cards with the horizontal arm white and the vertical black, and with the two arms of different colours.

Prepare two sets of cards, in the one of which the length of the total horizontal arm decreases by 1 cm. at each step, and in the other of which the vertical decreases in the same ratio, — until only the triangle of the mitre is left in each case.

Set the cards up at such a distance that the centre of the cross falls within the area of the blind spot. What is seen?

Unpractised observers generally assert that they see one of the arms going right across the other, and (for the most part) the horizontal,—perhaps because the horizontal diameter of the spot is less than the vertical. As the two arms are decreased, this judgment varies, until (with sufficient shortening) the smaller arm is entirely merged in the greater. Highly practised observers, on the other hand, declare that they are unable to decide how the centre of the figure is filled out; they cannot see the crossing-point at all.

We cannot here enter upon the theory of these phenomena. See Helmholtz, Physiol. Optik, 2d ed., 717–727; Aubert, Physiologie der Netzhaut, 257 f.; Wundt, Physiol. Psychol., 4th ed., ii., 103 f.; Hering, in Hermann's Hdbch., iii., 1, 374.

(2) *The Determination of the Macula Lutea.* — The pigmentation of the yellow spot may exert an effect upon colour vision. This is why *E* was told, in the campimetrical experiments, to start his fixation-mark, not from the centre of the opening, but from a point lying about 1 cm. from its centre.

There are various ways of bringing the yellow spot to vision, but the following are best adapted to showing its colour values.

Make a saturated solution of chrome alum. Filter. Dilute until the liquid is a pale slate blue. Pour into a flat-sided medicine bottle. Hold the bottle close before the eye, and look at a brightly illuminated window. The macula lutea appears as a small rose-coloured spot in the midst of the blue. The size of the spot depends upon the distance to which it is 'projected.' To the author, and to most of the *O*'s whom he has tested, it is about as large as a pea; other *O*'s declare that it is as large as a quarter-dollar.

The same experiment may be performed with purple gelatine sheets. — See Helmholtz, Physiol. Optik, 567 ff.; Hering, Pflüger's Arch., liv., 1893, 277; Sanford, Course, 100, 105.

EXPERIMENT III

§ 12. **Visual Contrast.** *Cautions not noted in the Text.* — Two things are necessary in these experiments: a power of steady fixation, and an ability to match the brightness of a colour and a grey. Experiment (1) gives good training in the former, if *O*

is not sufficiently trained by the foregoing campimetrical experiments. The matching of greys and colours is not altogether easy (*cf.* the matching of colours for brightness, in Exp. II.), though it is not either so difficult, after practice, as it may appear at first trial. Experiment (2) should afford sufficient practice, even if Exp. II. has not yet been performed. And the matching needed for our experiments is not of a very exact kind.

Throughout the experiments, *O* must be constantly on his guard against after-images. His decisions must be prompt; and a time of at least 3 min. should be left between judgment and judgment. It is well to agree on a pause of this duration beforehand, that *E* may not become flurried while adjusting the second disc to match the contrast colour.

All the colour-mixer experiments of this section are open to the charge that successive contrast is not excluded: *cf.* the theoretical objection to coloured papers in Exp. I. In practice, with a careful *O*, this source of error may be neglected.

EXPERIMENT (1). — As soon as the white card appears from beneath the 'black' strip, this strip suddenly darkens. *O*, keeping his fixation constant, is thus able to compare the brightness of the same 'black' upon a white and a black ground. The effect is very striking. — The experiment is described by Hering, Zur Lehre vom Lichtsinne, 23 f.

It is, of course, possible to *measure* contrast-effects of this kind. And we could get matches for induced brightnesses as, in the later part of this Experiment, we get matches for induced colours. But it is better to postpone this experiment until vol. ii., where it can be worked out methodically, and with the aid of photometrical formulæ.

(2) Every grey is tinged with the complementary colour. We use the tissue paper to eliminate contours. The experiment is described by H. Meyer, Poggendorff's Annalen, xcv., 1855, 170 f.; Phil. Mag., ser. 4, ix., 547.

It is commonly supposed that the chief effect of the tissue paper in this experiment is to lessen the saturation of the inducing ground; and that, consequently, we get better contrasts from less saturated than we do from more saturated colours. This proposition is negatived by exp. (4); but we can disprove it

here by a very simple variation of Meyer's experiment. Lay the grey over the coloured paper, without using the tissue: note the degree of contrast. Now lay the tissue on the coloured paper, and the grey on the tissue. The degree of contrast is lessened. — All that the tissue laid over *both* papers can do, then, by way of enhancing contrast, is to eliminate contours.

(3) The grey papers that are sold as grey are nearly always slightly tinged with brown or blue ('stone greys' or 'slate greys'). And it may not be possible to find a paper that is not noticeably lighter or darker than the coloured disc. *E* must do the best he can with his materials, noting the above points (if they are realised) as constant errors in his experimental series.

The distance of *O* from the discs must be such that the induced colour shows no marginal contrast. This distance will, if papers like those of Hering are used and the work is done in diffuse daylight, be somewhere about 2 m. *O* should fixate the centre of the disc, — not the coloured ring. Not only should the experiment be repeated with different colours, but for each single colour 5 tests should be made, if time allow, and the amount of the contrast-effect averaged from the separate results.

E is told in the text to work methodically. It will, however, materially shorten the experiment, and do no harm, if the Instructor give him some hint as to the composition of the contrast match. When he has made the general match roughly, *E* should first work for an exact *brightness* match, and only when this has been obtained seek to match the *colours* accurately. It is easier to vary sectors for colour, when the brightness is fairly good, than to vary them for brightness, after the colour match has been achieved.

The following results were obtained with the Hering discs:

(*a*) *Green* induces on ring of 120° W and 240° B a *Purple* of 40° Blue, 55° Red, 70° B and 195° W.

(*b*) *Red* induces on ring of 125° W and 235° B a *Verdigris* of 60° Green, 40° Blue, 80° B and 180° W.

(*c*) *Blue* induces on ring of 55° W and 305° B a *Yellow* of 31° Red, 60° Yellow, 200° B and 69° W.

(*d*) *Yellow* induces on ring of 270° W and 90° B a *Blue* of 24° Green, 71° Blue, 50° B and 215° W.

These figures will serve as a rough guide to the composition of the contrast matches, and to the brightness values of the coloured papers.

(4) The following Table of results (Hering papers) furnishes all the necessary comment upon this experiment:

Colour (inducing)	Ring	Colour (induced)
(*a*) Green	120° W, 240° Bk	40° B, 55° R, 70° Bk, 195° W
(*b*) 300° G, 20° W, 40° Bk	(20° W, 40° Bk) + 100° W, 200° Bk	30° B, 40° R, 71° Bk, 219° W
(*c*) 240° G, 40° W, 80° Bk	(40° W, 80° Bk) + 80° W, 160° Bk	23° B, 33° R, 71° Bk, 233° W
(*d*) 180° G, 60° W, 120° Bk	(60° W, 120° Bk) + 60° W, 180° Bk	20° B, 29° R, 72° Bk, 239° W
(*e*) 120° G, 80° W, 160° Bk	(80° W, 160° Bk) + 40° W, 80° Bk	17° B, 25° R, 72° Bk, 246° W
(*f*) 60° G, 100° W, 200° Bk	(100° W, 200° Bk) + 20° W, 40° Bk	12° B, 22° R, 73° Bk, 253° W
(*g*) 30° G, 110° W, 220° Bk	(110° W, 220° Bk) + 10° W, 20° Bk	6° B, 15° R, 75° Bk, 264° W
(*a*) Yellow	270° W, 90° Bk	24° G, 71° B, 50° Bk, 215° W
(*b*) 300° Y, 18° W, 42° Bk	(18° W, 42° Bk) + 225° W, 75° Bk	22° G, 56° B, 56° Bk, 226° W
(*c*) 240° Y, 36° W, 84° Bk	(36° W, 84° Bk) + 180° W, 60° Bk	18° G, 47° B, 59° Bk, 236° W
(*d*) 180° Y, 54° W, 126° Bk	(54° W, 126° Bk) + 135° W, 45° Bk	13° G, 30° B, 63° Bk, 254° W
(*e*) 120° Y, 72° W, 168° Bk	(72° W, 168° Bk) + 90° W, 30° Bk	11° G, 23° B, 65° Bk, 261° W
(*f*) 60° Y, 90° W, 210° Bk	(90° W, 210° Bk) + 45° W, 15° Bk	8° G, 20° B, 66° Bk, 266° W
(*g*) 30° Y, 99° W, 231° Bk	(99° W, 231° Bk) + 22.5° W, 7.5° Bk	5° G, 11° B, 67° Bk, 277° W

It is evident that the saturation of the induced colour varies directly with that of the inducing.

(5) This experiment is a good deal more difficult than any of the preceding. Despite the high degree of saturation of the marginal contrast colour, many observers are unable, without practice, to keep the attention upon the coloured line steadily enough to compare it with the other disc. Complementary after-images may be exceedingly troublesome here: the *O* of the results quoted below referred to them as 'satanic.' It is well to give *O* a head-rest, and to insist very strongly that fixation is not to wander from the centre of the smaller disc. Prompt judgment is indispensable.

Under such circumstances it is advisable to repeat each test some 6 times over, and to average the results. With a good observer, however, the variation from test to test is but slight.

The following Table shows some marginal contrasts, obtained with Hering discs, and compares them with the diffused contrasts obtained with the same discs in experiment (3):

(*a*) Green.	Brightness match: 120° W, 240° Bk.		
	(1) Diffused purple induced:	40° B, 55° R,	70° Bk, 195° W.
	(2) Marginal " "	47° B, 80° R,	56° Bk, 177° W.
(*b*) Yellow.	Brightness match: 270° W, 90° Bk.		
	(1) Diffused blue induced:	24° G, 71° B,	50° Bk, 215° W.
	(2) Marginal " "	30° G, 90° B,	40° Bk, 200° W.
(*c*) Red.	Brightness match: 125° W, 235° Bk.		
	(1) Diffused verdigris induced:	60° G, 40° B,	80° Bk, 180° W.
	(2) Marginal " "	74° G, 40° B,	80° Bk, 166° W.
(*d*) Blue.	Brightness match: 55° W, 305° Bk.		
	(1) Diffused yellow induced:	31° R, 60° Y,	200° Bk, 69° W.
	(2) Marginal " "	35° R, 80° Y,	198° Bk, 47° W.

(6) There is little difficulty in this experiment, beyond the difficulty of manipulating the discs. It is well to have several sets ready, with the various rings pasted on beforehand.

The following results are characteristic:

Inducing colour: Green

Ring	Induced colour
(*a*) 120° W, 240° Bk	40° B, 55° R, 70° Bk, 195° W
(*b*) 90° W, 270° Bk	27° B, 37° R, 143° Bk, 153° W
(*c*) 60° W, 300° Bk	18° B, 27° R, 250° Bk, 65° W
(*d*) 30° W, 330° Bk	15° B, 18° R, 290° Bk, 37° W
(*a*) 150° W, 210° Bk	30° B, 50° R, 62° Bk, 218° W
(*b*) 180° W, 180° Bk	20° B, 45° R, 56° Bk, 239° W
(*c*) 210° W, 150° Bk	15° B, 42° R, 50° Bk, 253° W
(*d*) 240° W, 120° Bk	10° B, 41° R, 48° Bk, 261° W
(*e*) 270° W, 90° B	8° B, 38° R, 46° Bk, 268° W
(*f*) 300° W, 60° Bk	6° B, 35° R, 44° Bk, 275° W
(*g*) 330° W, 30° Bk	4° B, 32° R, 40° Bk, 284° W

It is clear that the contrast-effect decreases with introduction of brightness contrast, whichever direction this may take. There is good reason, then, for our care to avoid brightness contrasts in the previous colour experiments.

(7) and (8) Neither of these experiments is quite easy. *O* will have grown so used to the rotating discs, that the contrast-colour of the stationary disc will be hard to estimate. And the

tissue-paper front of the discs in (8) is distracting to the attention. On the other hand, the effects are very striking, when once O is sufficiently trained to observe them. The tissue-covered disc seems rather to be transmitting than to be reflecting light; the contrast colour seems purer, as it were a more positive colour, than it has done in previous experiments.

The following results are characteristic:

(*a*)	Green,	without	tissue	paper,	induces	40° B,	55° R,	70° Bk,	195° W
(*b*)	"	with	"	"	"	48° B,	75° R,	45° Bk,	192° W
(*c*)	Red,	without	"	"		60° G,	40° B,	80° Bk,	180° W
(*d*)	"	with	"	"	"	80° G,	55° B,	50° Bk,	175° W
(*e*)	Blue,	without	"	"	"	31° R,	60° Y,	200° Bk,	69° W
(*f*)	"	with	"	"	"	35° R,	78° Y,	112° Bk,	135° W
(*g*)	Yellow,	without	"	"		24° G,	71° B,	50° Bk,	215° W
(*h*)	"	with	"	"		27° G,	92° B,	40° Bk,	201° W

It will be seen that the values of exp. (8) equal or exceed the values obtained for marginal contrast in exp. (5). This is the rule, — although some observers give relatively higher values to the marginal contrasts than were given by the O of the results cited.

It may be mentioned here that all the results of this Experiment were obtained from the same O, under (as nearly as possible) like conditions of illumination. The conditions, indeed, cannot have varied much: for the brightness match of the standard colours was tested before every partial experiment, and (as the quotations show) remained true throughout.

Questions. — (1) Contrast is present at once, as soon as the discs are displayed. If a contrast disc be set in rotation, covered by a screen, and then suddenly shown to a person who is ignorant of its actual composition, it will be described, without hesitation, as bicoloured.

(2) Precisely the same laws would hold. Each colour would modify the other in the direction of its own antagonistic colour; the contrast-effect would vary with saturation, presence or absence of brightness contrast, distance of the contrasting surfaces, contours.

E may, if time allow, make up some contrast discs of two colours, instead of a colour and a grey, and show them to O.

O will describe exactly what he sees, and *E* can work out these introspective results in the light of the five laws of contrast.

(3) Coloured shadows fulfil these conditions. The principle of the experiment is as follows. Two shadows of an upright wand are thrown, side by side, upon a screen, by means of a light and a mirror placed to one side of it. Between light and wand a deeply coloured (*e.g.*, red) glass is set up. The shadow due to the reflected light, being illuminated by the red rays, shows an intensive red colour: the other shadow, due to the direct light, shows an equally intensive blue-green. The common background of both shadows is the mixture of red and of (comparatively) white light. The saturation of the red shadow is thus somewhat diminished; while the contrast colour is, so to say, all marginal contrast (and therefore intensive), and gains further by the complete merging of the shadow contour in the texture of the screen.

(4) See Helmholtz, Physiologische Optik, 2d ed., 542, 564; Hering, Zur Lehre vom Lichtsinne, 21 ff. See also Hering, Ueber die Theorie des Simultankontrastes von Helmholtz, Pflüger's Archiv, xl., 1887, 172; xli., 1887, 1, 358; xliii., 1888, 1. A good account is given by Ebbinghaus, Psychologie, i., 224 ff.

(5) Hering, as above.

LITERATURE. — To the general references given above add Wundt, Phys. Psych., i., 518. For work upon the measurement of the contrast-effect, see H. Ebbinghaus, Sitzungsber. d. Berliner Akad., xlix., 1887, 995; C. Hess and H. Pretori, Arch. f. Ophthalmologie, xl., 4, 1894, 1; A. Kirschmann, Philos. Stud., vi., 1890, 417; A. Lehmann, Philos. Stud., iii., 1886, 497; H. Pretori and M. Sachs, Pflüger's Archiv, lx., 1895, 71.

INSTRUMENTS. — The most beautiful contrast-effects with which the author is acquainted are given by the Hering window (Rothe, Mk. 150). A large black screen, having two oblong openings, is fixed over the window (or let into the wall) of the dark room. The one opening is filled with a ground glass; the other with a red, orange, green or blue glass. The openings may be narrowed and widened at pleasure. The two shadows of a black rod are thrown upon a screen of milk glass. It is easy so to arrange the openings that *O*, looking at the screen and not at

the window, shall declare the contrast-colour to be the richer, more saturated and more positive colour of the two.

On discs for the demonstration of contrast, see Helmholtz, Phys. Optik, 544 f.; Sanford, Course, 158; Aubert, Phys. Optik, 497.

Hering (Pflüger's Arch., xli., 1887, 358) describes an instrument (Rothe, Mk. 28) for experiments upon mirror contrast ('Ragona Scinà's experiment:' see D. R. Scinà, Atti dell' Acad. Palermit., iii., 1859; Helmholtz, Phys. Optik, 557; Wundt, Phys. Psych., i., 424; Sanford, Course, 155 f.). Two other instruments of Hering's for the demonstration of simultaneous contrast are sold by Rothe at Mk. 50 and Mk. 30 respectively.

EXPERIMENT IV

§ 13. **Visual After-images.** (1) *Negative.*—This experiment follows Hering, whose Zur Lehre vom Lichtsinne should be read (or lectured upon) alongside of the laboratory work. The alternative theory is that of Fechner: that the phenomena of negative after-images are explicable in terms of retinal fatigue (*verminderte Reizempfänglichkeit*). This hypothesis is adopted by Helmholtz, in the Physiol. Optik; and the student should be told that Helmholtz asserts, in his 2d edition: "I have so far been unable to discover any phenomenon that is distinctly irreconcilable with Fechner's principles of explanation." A strong statement, and a statement made after full survey of "dieses äusserst verwirrte Gebiet der mannigfaltigsten Erscheinungen"! Nevertheless, the author has become convinced by repeated experiments that Hering's account of simultaneous and successive contrast, and simultaneous and successive light-induction, is very much more adequate to the phenomena, taken as a whole, than is the treatment in the Physiol. Optik. James speaks truly, when he says of the book: "It seems to me that Helmholtz's genius moves most securely when it keeps close to particular facts." If, then, the student is to theorise his facts as he goes, if he is not merely to 'observe' the after-images and have done with them, the Instructor has, in the author's judgment, no choice but to follow Hering.

General references are: G. T. Fechner, Poggendorff's Ann., xliv., 1838, 221, 513; l., 1840, 193, 427; Helmholtz, Phys. Optik, 2d ed., 501 ff., 537 ff.; H. Aubert, Phys. d. Netzhaut, 364; Wundt, Phys. Psych., 4th ed., i., 512 ff.; C. Hess, Arch. f. Ophthalmologie, xxxvi., 1, 1890, 1 ff.; O. N. Rood, Students' Textbook of Colour, 1881, 235 ff.; Hering, Zur Lehre vom Lichtsinne, 1878; Pflüger's Arch., xlii., 1888, 488; xliii., 1888, 264, 329; Ebbinghaus, Psych., i., 230 ff.

Preliminary Exercises. — (1) At first the room is oppressively black; but in the course of the first two minutes it has considerably lightened, and at the expiration of ten minutes or a quarter of an hour is a steady dull grey. The observer should note the various 'subjective' phenomena produced: flashes or points of colour, more especially of yellowish grey; bright grey clouds of varying form and extent, etc. — Aubert, Physiol. d. Netzhaut, 1865, 27, 39. (2) At first one is dazzled; it is painful to keep the eyes open; one blinks, and instinctively seeks the shadowy places in the light room. In the course of a minute the eyes have become accustomed to the light. (3) The colour is, at first, distinctly yellow; but we soon become as indifferent to this yellow as we are to the reddishness of ordinary daylight. (4) At first the colour of the glass is very noticeable. At the end of the five minutes there will be hardly a trace of it remaining. — A good variation of the experiment is to have spectacle-frames (of the kind supplied with side-pieces or temple-frames) filled with the differently coloured glasses, and to let *O* wear the spectacles for an hour or two. Complete adaptation is thus secured.

(5) This is Hering's experiment, Lichtsinn, 36. For the first few seconds, the adjoining black and white are enhanced by contrast. Very soon, however, the black is covered by a grey veil, which gradually lightens, and the white by a grey shadow, which gradually darkens. Both veil and shadow move outwards from the line of junction of the two surfaces, where they also remain most distinct throughout the experiment. From time to time, owing to unsteadiness of fixation, there appears along the line of junction a streak of brilliant white or of deep black, the white brighter, and the black more intense, than the white and black of the two surfaces. The streaks belong to the negative after-

images of the surfaces. They are indifferent to the course of the present experiment, except that the black streak on the black surface and the white on the white surface serve, by contrast, to emphasise the greyness to which the two brightnesses have been brought by 'simultaneous light-induction.'

(6), (7) The result is the same, *mutatis mutandis*, as in (5). The general law of adaptation is exemplified in each case.

(8) The general effect will be that the small disc 'goes out,' is merged in the colour or grey of the background. The particular effect — the colour changes which the disc passes through, the quickness with which it disappears, the change of the background itself in the neighbourhood of the disc, etc. — varies with the quality and intensity of the disc and background, with the size of the disc, with the foregoing adaptation of the retina, etc., and seems, besides, to evince individual variations of which no explanation can at present be offered.[1] Ebbinghaus, Psych., i., 234; G. T. Ladd, Yale Studies, vi., 1898, 1.

On adaptation, see Helmholtz, Phys. Optik, 508, 555 ff.; A. Fick, in Hermann's Handbuch d. Physiol., iii., 1, 1879, 222 ff.; Aubert, Physiol. Optik, 1876, 483 ff., and *op. cit.*; T. Treitel, Arch. f. Ophthalm., xxxiii., 2, 1887, 73; A. E. Fick and A. Gürber, *ibid.*, xxxvi., 2, 1890, 245; A. E. Fick, *ibid.*, xxxviii., 1, 1892, 118; xxxviii., 4, 1892, 300; Hering, *ibid.*, xxxvii., 3, 1891, 1; xxxviii., 2, 1892, 252; Pflüger's Arch., liv., 1893, 277; A. Charpentier, La lumière et les couleurs au point de vue physiologique, 1888, 154 ff.[2] On the apparent difference between the shift of brightness and the shift of colour, see Hering, Lichtsinn, 89.

MATERIALS. — A skeleton standing-desk can be made very cheaply, and is of great service in the laboratory. Useful dimensions are: height, front, 1.20 m., back, 1.35 m.; desk surface, 55 by 80 cm. A low edge should run across the front of the desk; and a flat shelf may be nailed to the back. It is, of course, better to have the desk too high than too low.

[1] Cf., however, M. F. McClure, Amer. Journ. of Psych., Jan., 1901.

[2] The literature of this Section is very voluminous. The following list of names may be of assistance to the Instructor for further reference: S. Bidwell, H. P. Bosscha, H. Ebbinghaus, C. Hess, J. von Kries, T. Lipps, J. E. Lough, G. Martius, A. Tschermak, W. Uhthoff, H. Voeste, W. Wirth.

Experiment (1). — In this and the following experiments, where no specific directions as to illumination are given, a moderate diffuse daylight is presupposed: strong light is to be avoided. Both eyes are used for fixation, which should be as steady as possible: winking, eye-movement, etc., are disturbing factors.

O will probably report incidental colour-effects, oscillations of brightness, temporary disappearances, apparent movements, etc., in the after-image. All these changes should be noted by *E*, although they are indifferent to the present experiment. The constant phenomena are (*a*) a darker disc, sharply outlined against the dark background of the closed eyes, and surrounded (*b*) by a halo of light, which is brightest where it touches the disc, and gradually decreases in brightness towards the periphery, till it is lost in the darkness of the general field. The halo is a phenomenon of successive light-induction: Hering, Lichtsinn, 5, 19.

The time of fixation must be determined in preliminary trials. It is not necessary, but it is useful as practice for later work, that *E* should have a stop-watch. He starts the watch at a "Now!" which is the signal for *O*'s fixation; at the end of the 20 or 30 sec. he gives a second "Now!" which is the signal for *O* to close his eyes; and as *O* reports the course of the after-image, he jots down the time at which the various phases appear. The time-order of the phenomena, as thus recorded, has no great scientific value: *O* is unpractised, and the adaptation of his eyes will probably vary somewhat from experiment to experiment. But the record will accurately represent the course of the image as an individual phenomenon, and (as was said above) *E* will gain in practice.

The following variation of the experiment brings out very forcibly the 'physiological' character of the halo. Lay on the velvet two 1 cm. squares of white paper, 4 mm. apart. Fixate a pin-head or other small object set in the middle of the black strip. Observe the after-image of the squares as before. *O* sees (*a*) two deep black squares, surrounded (*b*) by their halos; but — and this is the point — the middle strip, where the two halos coincide or overlap, is very much brighter than the halo round the remaining three sides of the squares.

Hering, Lichtsinn, 9; Sanford, Course, 161, exp. 154.

Experiment (2). — *O* will probably report incidental colour-effects, narrow and variously coloured edges, differences of

brightness at different parts of the image, alternation of phase, etc. The constant phenomena are (*a*) the intense brightness of the after-image strip, and (*b*) the absence (or very vague and weak character) of the dark halo which the analogy of the foregoing experiment has suggested. — Hering, Lichtsinn, 11 f.

EXPERIMENT (3). — A careful *O* will see, in the moment after the removal of the white disc, a positive (weakly grey) after-image. This immediately makes way for the negative effect: (*a*) a deeper black disc, surrounded (*b*) by a lighter fringe. — Hering, Lichtsinn, 98.

EXPERIMENT (4). — *O* sees (*a*) a white strip, much more intense than the white background, surrounded (*b*) by a dirty-white or bright grey fringe, the 'dark halo.'— Hering, Lichtsinn, 98 f.

EXPERIMENT (5). — The after-image is a dark grey.

In the alternative experiment, the white strip grows slowly duller during fixation (simultaneous light induction). When the black pieces are removed, the strip, which is still white, turns suddenly to a dark grey. Hering, Lichtsinn, 97.

EXPERIMENT (6). — The after-image is whitish.

In the alternative experiment, the black strip is at first very dark, and grows gradually lighter (simultaneous light induction). When the white pieces are removed, it appears at once as whitish (successive light induction). Hering, Lichtsinn, 97 f.

EXPERIMENT (7). — The constant phenomenon is a coloured after-image whose colour is complementary to that of the stimulus disc.

The duration of the after-image may be measured as before.

A control experiment may be performed as follows. Use for the coloured discs papers whose complementaries have been determined in Exp. I. Set up the complementary discs upon the colour mixer, in front of a grey screen of the same brightness as the grey background of the wooden frame. Cover the mixer by another grey screen until the after-image has developed: then suddenly expose the complementary mixture, and let *O* compare, by a quick glance, the tone of the after-image with the tone of the revolving discs. There will be incidental differences (the after-image will appear limpid, the discs 'thick'; there may be difference of saturation, etc.); but if the experiment has been carefully performed, the colour-tones will give a fair match. On sources of error, see Hering, Lichtsinn, 127 f.

Experiment (8). — The result here is precisely the same as in the case of contrast. The complementary colours of the after-image mix with the colour of the 'reacting surface' according to the general laws of colour mixture, and the resulting image is as clear as is the image on a neutral ground. — Sanford, 154, exp. 151.

Question (1) Whether or not any of the coloured discs of exps. (7) and (8) show a contrast-effect will depend upon circumstances: the nature of the papers used, the general illumination, the brightness of the surrounding grey surface. It may be, *e.g.*, that the red disc gave a trace of the contrast green when it was first fixated. As fixation continued, the green disappeared, and the grey in the near neighbourhood of the disc took on a reddish shimmer (induction). In the after-image, however, the complementary green disc was surrounded by a clear reddish halo. This proves both that the contrast-sensation is effective, and that the contrast-effect is enhanced.

Hering gives the following as an instance of the after-image of a contrast sensation. Cut two strips of dead-finish dark-grey (imperfectly black) paper, 4 cm. long and .5 cm. wide. Prepare a background, half white and half black, of baryta paper and velvet laid side by side. Place the strips upon the background, the one upon the white and the other upon the black surface, laying them parallel to the line of junction and at a distance of at least 1 cm. from it. Fixate a pin-head, set between the strips in the line of junction, for 30 to 60 sec. At the beginning of the experiment, the one strip looks much brighter than the other; as fixation is continued, this brightness-difference gradually diminishes.

After fixation, close the eyes and cover them with the hands. The bright portion of the background is dark, and the dark portion light, in the after-image. Moreover, the strip which at first looked brighter is now darker, and the strip which looked darker is brighter, although the objective brightness of the two strips was the same. The contrast-effect is, then, reflected in the after-image. And, in general, the brightness-difference of the strips in the image is very considerably greater than their contrast-difference in the stimulus.

Notice that, when the after-image has so far faded out that the brightness-difference of the field has disappeared, the difference between the strip-images may still persist, the one being brighter and the other darker than the uniform background. — Lichtsinn, 24 f., 27.

(2) This question has been answered by the foregoing experiments, and also by the alternative experiments (5) and (6) of the text. A pretty illustration (Ebbinghaus) is as follows. Lay two moderately large sheets of saturated green paper upon a grey ground, leaving a horizontal strip of .5 cm. width between them. *O* fixates a mark in the centre of the grey strip. After 30 sec. he projects the after-image upon an irregular surface, *e.g.*, upon the nearest window-frame. The after-image is almost invariably described as a 'green strip.' The red after-image of the field is lost in the irregularities of the reacting surface, while the green 'heaped up' over the grey strip in stimulus and after-image is sufficiently strong to draw the attention exclusively to itself. — Psych., i., 239.

(3) No extended series of experiments can be made that does not furnish evidence of the periodicity of the after-image. It comes and goes; the relative brightness of its parts varies from appearance to appearance; some parts persist unchanged, while others merge in the general background. Many of the changes are, doubtless, due to movement of the eyes, unnoticed changes in the illumination of the reacting surface, wandering of the attention: Helmholtz has pointed out that even a change of breathing may affect the after-image. But over and above these accidental influences, there is an uniformity in the phenomena which points distinctly to a periodicity grounded in the nature of the after-image itself.

On the positive side, see Hering, Lichtsinn, 44; Aubert, Phys. d. Netzhaut, 373 ff.; Phys. Optik, 514. On the negative, Fechner, Poggendorff's Annalen, xliv., 1838, 525; Helmholtz, Phys. Optik, 510.

EXPERIMENT (9). — Although this experiment is quantitative in character, it affords an excellent means of studying the qualitative course of the image, and so has its justification in the pres-

ent volume. The apparatus is somewhat cumbrous: but it is not expensive, and will serve a number of purposes in later laboratory work.

The words 'relative' and 'absolute' in the formulation of the law may need explanation. The law says that the intensity and duration of the image depend (*a*) upon the intensity of the stimulus, (*b*) upon the intensity of the stimulus as compared with the intensity of its surroundings, (*c*) upon the duration of the stimulus, (*d*) upon the intensity of the light-surface upon which the image is projected, and (*e*) upon the intensity of this reacting light as compared with the intensity of the primary stimulus. All five factors may be suitably varied with the described apparatus.

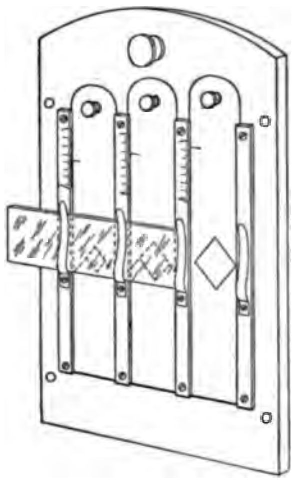


FIG. 3. — Aubert diaphragm.

The apparatus itself may be modified in a great variety of ways. On the side of cheapness, we may substitute oil-lamps for the burners, hand-screens for the gas-cocks, and coloured glasses for the gelatines. On the other side, we may have an Aubert diaphragm (Phys. Optik, 547; Phys. d. Netzhaut, 44), in place of the circular opening in the screen; and may use, instead of the ordinary gas-cocks, the cocks supplied with a bar-handle and graduated arc. The limits through which the handle is to be turned in the manipulations may be marked by lumps of wax squeezed down upon the arcs, or pieces of electric tape wrapped round them. If ordinary cocks are employed, the limits may be set by wire nails driven into the table. A blackened observation tube may be useful.

It may be mentioned, as a point of method, that some *E*'s have great difficulty in taking notes and counting the metronome strokes at the same time. If the difficulty is not overcome by practice, the counting may be done by *O*. As the image changes, *O* calls out catch-words which *E* puts down; when the image has disappeared, *E* reads these words to *O*, who amplifies his introspections and gives the times at which the changes occurred.

On the use of coloured gelatines, see A. Kirschmann, Philos. Studien, vi., 1891, 543. For another apparatus, see S. I. Franz, Psych. Rev. Monograph Suppl. 12, 1899; for apparatus on the lines of that here recommended, see E. W. Scripture, Philos. Studien, vii., 1892, 53; E. B. Titchener, *ibid.*, viii., 1893, 247; W. B. Pillsbury, Amer. Journ. of Psych., viii., 1896–7, 343.

The following are typical results. The individual character of such a series need not be emphasised.

Stimulus: red. Left eye. Reacting surface moderately bright. 3 min. intervals.

Stimulus:	Image:	
5 sec.	12 sec.	Green disc, with violet (at first merely bright) halo: 7 sec. Green. Greenish grey.
10 "	18 "	Red centre (3 sec.), green disc, red halo: 7 sec. Green. Dark greenish grey.
15 "	23 "	Violet centre (3 sec.), green disc, reddish violet (later bright) halo: 10 sec. Green. Greenish grey.
20 "	25 "	Bright violet centre (3 sec.), green disc, violet (3 sec.: later bright green) halo. Dark green, with bright green halo.
25 "	30 "	Violet disc. Green disc, with violet-red (9 sec.: later bright green) halo. Dark green.

Where a positive image precedes the negative, there is usually an intervening period, perhaps of 2 or 3 sec., during which no image is seen.

Question (4) See, besides Helmholtz and Hering, *opp. citt.*, Ebbinghaus, Psychologie, i., 248 f., 251 f.

(5) Ebbinghaus, Psychologie, i., 235, 258.

FURTHER EXPERIMENTS. — The following experiments present points of interest.

(i.) *Observation of the Negative After-image with Persistence of the Stimulus.* — Arrange the apparatus as for exp. (9), leaving aside the second dark-box and burner. Expose a stimulus — *e.g.*, red — for the usual time. Then simply turn down the stimulus-burner to a low intensity of illumination, so that the red, as seen by the normal eye, would appear as a dark reddish-brown. The originally red disc is transformed, for *O*'s eyes, into a complementary green.

The experiment may be performed more simply as follows. Set up, in moderate diffuse daylight, a small red disc on a yellow ground. *O* fixates the centre of the red disc for the usual time. Then a curtain is suddenly drawn across the illuminating window, so that the general brightness of the stimulus is considerably decreased. *O* sees a green disc on a blue ground. — Ebbinghaus, 238; Sanford, Course, 113, exp. 124; C. L. Franklin, Mind, N. S., ii., 1893, 485.

(ii.) *Change in the Apparent Magnitude of the After-image with Distance of the Reacting Surface.* — Secure a strong nega-

tive after-image as in exp. (7). Project it successively upon a series of grey backgrounds set up at different distances from the eyes. Note that the after-image enlarges as the distance increases. — Aubert, Phys. d. Netzhaut, 367.

Project the after-image also upon curved or bent backgrounds, and note that it seems itself to curve or bend correspondingly: Sanford, Course, 112, exp. 124.

(iii.) *Movement of the After-image with Movement of the Eye.* — Get a strong negative after-image with both eyes, and note that it moves to all parts of the room as the fixation-point changes. When the eyes are steady (when, *e.g.*, *O* fixates a pencil point) the after-image remains stationary.

Secure a monocular after-image a little to one side of the fovea: *i.e.*, fixate monocularly the central pin-hole in the front screen of exp. (7), and pin a coloured disc a few cm. to right or left of it. Open both eyes, and project the after-image to various parts of the room. Note the recurrence of the effort to fixate the image, and its entire fruitlessness. — Helmholtz, 507; Aubert, 367.

Note the effect upon the after-image of unsteady fixation, of incomplete adaptation, of winking and eye-movement during its course, of winking immediately after its disappearance. — Ebbinghaus, 240 f.

(2) *Positive After-images.* — The phenomena of the positive after-image are not yet fully understood, and the recorded observations are not all in agreement. Neither of the two dominant theories of visual sensation has offered any satisfactory principle of explanation. The current view, that the positive after-image represents a simple persistence of stimulation, is certainly inadequate.

References: Aubert, Phys. d. Netzhaut, 347; Helmholtz, Phys. Optik, 480, 503; C. Hess, Pflüger's Arch., xlix., 1891, 190; S. Bidwell, Proc. Roy. Soc., lxvi., no. 337, 1894, 132; A. Charpentier, Arch. de physiol., sér. 5, iv., 1892, 541, 629.

Experiment (10). — With some practice, the observer is able to see a positive after-image, true in brightness and colour to the original. The details of the scene — the leaves of the shrubs, the string and tassel of the window-shade, etc. — come out with surprising clearness.

A pretty variation of the experiment may be made with artificial light, as follows. Seat yourself at a table which is covered with variously coloured objects and lighted by a good lamp. Proceed as in the previous experiment with the window. When the field is darkened, for the projection of the after-image, the hands seem for a moment to be actually transparent, so vivid is the appearance of the coloured images. Many details may be observed in the after-image which escaped observation during the rapid glance at the real objects.

Note that the darker surfaces disappear first, without any considerable change of colour. The brighter surfaces remain longest, and undergo a regular series of colour changes. — Helmholtz, Phys. Optik, 504.

EXPERIMENT (11). — Helmholtz, as before. The experiment may be varied by observing the light through variously coloured glasses or gelatine-mixtures.

EXPERIMENT (12). — The irregularly shaped after-image is positive upon the dark field, and negative upon the bright field.

The look at the sun must be extremely short. After-image experiments are always trying to the eyes, and the stimulation received from the solar disc is, of course, extraordinarily intensive. Helmholtz advises that only a few after-image experiments be made on any given day, and that the experiments be discontinued if the observer complains of pain in the head or eyes, or experiences such pain when looking at bright or vividly coloured surfaces, or even begins to have unusually persistent and vivid after-images. Phys. Optik, 502 f.; *cf.* Aubert, Phys. d. Netzhaut, 371.

EXPERIMENT (13). — The after-image is positive on the dark, and negative on the white field. In exp. (12), the stimulus is very brief, but of very high intensity; in exp. (13) the stimulus is not overbright, but is continued for a longer time than suffices for the arousal of a positive image. The time of stimulation in experiments of the type of (10) and (11) is ordinarily given as 0.3 sec. Helmholtz, 503.

EXPERIMENT (14). — The moving red point is continued as a red streak, due to the slowness with which the primary sensation rings off. The red streak ends as a grey, or greyish red, which is continued, in its turn, as a blue-green streak, showing

bright against the dark background (positive and complementary after-image). Some observers report a blank interval between the red and the blue-green; the red then ends abruptly, without becoming grey. After this blue-green streak would come, if the full series of phenomena were represented, the red positive after-image of the point; and after that, again, the (dark) blue-green negative image. Ebbinghaus, Psych., i., 244; Sanford, Course, 114, exp. 125; first described by J. Purkinje, Beobachtungen u. Versuche z. Phys. d. Sinne, ii., 1825, 110.

Experiment (15). — This experiment shows, in a striking way, the effects of practice. The report of a wholly unpractised observer is a mere chaos. With attention, the uniformity of the phenomena soon becomes apparent; and presently the observers who at first gave radically different accounts of the after-image will reach agreement upon all essential points.

With an unclouded sky, or a sky thinly covered with clouds and presenting an even white surface, the flight of colours is as follows:

(*a*) A momentary positive and same-coloured image.

(*b*) Interval of 5 or 6 sec.

(*c*) Positive image, fluctuating in colour; sometimes with patches of red and green. After 1 or 2 sec., the image settles down to a sky ***blue***, the vertical bar remaining dark.

(*d*) The blue passes, with or without interruption, into a ***green***. The green is at first very vivid; it disappears and reappears five or six times, growing gradually paler; at last it is almost whitish. — These initial changes show a good deal of individual variation. Some *O*'s now see

(*e*) A *yellow* image. This (or the whitish green preceding) is regularly followed by

(*f*) A deep *red* image. The black bar becomes luminous and slightly greenish, the light appearing first as a crack in its length. This is the stage of transition from the positive to the negative image. The red undergoes several fluctuations. Then follows

(*g*) A deep *blue* image, with yellowish bright bar, more lasting than any of the preceding phases. The blue darkens, and the image gradually disappears, with or without passing into

(*h*) A dark *green* image. — Helmholtz, Phys. Optik, 524; M. F. Washburn, Mind, N. S., viii., 1899, 25, and unpublished experiments. Note the periodicity of stages *c* to *h*:

B — G — Y — R — B — G

Question (6) If the illumination is much diminished, the sky dull and heavily clouded, stages (*c*), (*d*) and (*e*) are lacking. The first image is a reddish white, with dark bar. The red gradually deepens, and the negative image appears, followed after several fluctuations by a dark blue negative image.

(7) After a certain limit has been passed, the duration of the stimulus does not affect the course of the image. Try with 10 and 15 sec. exposures. Helmholtz, 524. — With very brief stimulation, the sequence is: (*a*) white, passing quickly through (*b*) greenish blue to (*c*) deep blue, and then into (*d*) violet or rose. Then follows (*e*) a dull orange, during which the image may change from positive to negative, and become a dirty yellow-green. Helmholtz, 521 f.

(8) The usual explanation (Plateau, Fechner, Helmholtz, Wundt) is that "the after-effect of the excitation is dependent upon the wave-length of the light." The white light of the window is broken up into its physical constituents, and the temporal course of the red, green and violet excitations in the retina (or the visual apparatus) shows characteristic differences. See, *e.g.*, Helmholtz, Phys. Optik, 522; Wundt, Phys. Psych., i., 516. Hering believes that there is always some colour, however weak, in the original stimulus: Lichtsinn, 85, 112, 125. The periodicity which appears in exp. (15) and in the answer to Question (7) is strongly suggestive of Hering's general theory of colour vision.

(3) *Binocular After-images.* — The question of a functional interconnection of the two retinas (or of the two halves of the visual apparatus) is as old as Newton, and has been very variously answered. The latest writer on the subject, Franz (Psych. Rev. Mon. Suppl. 12, 1899, 44), takes a negative standpoint. The author, however, regards Franz' criticism as inconclusive ('suggestion,' *e.g.*, may work as well against as for the binocular image); and finds in the assumption of such an interconnection the only means of explaining the observed facts.

Experiment (16). — As soon as the left eye is opened, *O* sees upon the white ground a faint reddish image, fairly clear in outline. After 1 or 2 sec., the middle portion of the ground suddenly darkens, and (after about 1 sec.) there appears upon it a

complementary (blue-green) after-image. Shade and image remain for some little time. Then the ground clears again, and, under favourable conditions (no eye-movement, complete adaptation, moderate illumination), the original reddish image reappears as a shapeless patch. Very soon the darkening of the field and the complementary image recur. — The author has been able to see the red image three times, and the complementary image on the dark ground five times, in a single experiment.

The darkening is due to the superposition of the field of the closed right eye upon that of the left. The complementary image belongs to the right eye: it appears only upon the darkened field. The red image is the effect of the indirect stimulation of the left eye (or left half of the visual apparatus).

S. I. Franz, *op. cit.*; Sanford, Course, 116, exp. 127; 175, exp. 169; Titchener, Philos. Studien, viii., 1893, 244 ff. The author regards the results of the α-method in this paper as reliable; the β-method he now considers untrustworthy.

The following variation of the experiment (Franz) is instructive. Fixate the ink dot on the white surface, and lay a small disc of bright orange paper in such a position that it is altogether lost in the blind-spot area of the right eye. Secure the head firmly by head-rest and mouth-board (p. 245). After adaptation, open the left eye, and fixate the ink dot for 5 sec. Then close the left, and open the right eye. No image appears, until the field has darkened (superposition of left field), when a dim blue disc is seen in indirect vision. In other words, the left-eye image is seen, but there is no right-eye (transferred) image. This result points toward a direct functional interconnection of the retinas: for, if the 'binocular' image were merely a matter of central excitation, there is no reason why it should not be seen within the blind area of the right eye.

The experiment must be checked by experiments of the type of (16). A valid result presupposes a high degree of practice on the part of *O*.

INSTRUMENTS. — Wundt's after-image apparatus (Phys. Psych., i., 543; Zimmermann, Mk. 60) is excellent for demonstration purposes. The instrument recommended for exp. (7) is a simple form of Hering's apparatus (Rothe, Mk. 45).

CHAPTER II

Auditory Sensation

§ 14. **Auditory Sensation.** — On simple tone and simple noise see:

A. Barth, Zur Lehre von den Tönen und Geräuschen. Zeits. f. Ohrenheilkunde, xvii., 1887, 81.

H. Ebbinghaus, Grundzüge d. Psychologie, i., 1897, 276.

H. von Helmholtz, Sensations of Tone, 1895, 8, 23, 145, 150.

A. Höfler, Psychologie, 1897, 95.

E. C. Sanford, Course, exps. 63, 64, 65, 66, 69.

C. Stumpf, Tonpsychologie, i., 1883, 178, 189; ii., 1890, 257, 511.

W. Wundt, Grundzüge d. physiol. Psychologie, i., 1893, 443.

Cf. also Foster, Textbook of Physiol., iv., 1891, 1361; Stout, Manual of Psych., 171; Titchener, Outline, 57; Külpe, Outlines, 102; V. Hensen, in Hermann's Handbuch d. Physiol., iii., 2, 1880, 3.

Sensations of tone are, perhaps, of all sensations, those which the average student approaches with the greatest diffidence and the least interest. It has often been said that the Anglo-Saxon peoples are unmusical; and, although general statements of this kind should not be lightly accepted, there can, at least, be no doubt that the world of tones receives but a minimal attention as compared with the world of colours. We are always thinking about 'how we look'; it occurs, apparently, to very few to think how they sound. But, over and above this lack of practice in the apprehension and discrimination of tones, there is a wide-spread belief that tone psychology presupposes musical gifts and musical training. "There is a close relation," says Külpe, "between tonal fusion and the tonal connections whose æsthetic effect is displayed in music. We are thus in the fortunate position of having the results of centuries of artistic practice to compare with the outcome of psychological experimentation." Now while this is true, — while an investigation

of the complex formations, melody, consonance and dissonance, etc., does demand musical knowledge, and would not be undertaken by students who did not possess such knowledge, — still there is no need of musical ability for the fundamental experiments in tonal sensation. Even in such a matter as the analysis of clangs into their partials, "a musically trained ear will not necessarily hear upper partial tones with greater ease and certainty than an untrained ear. Success depends rather upon a peculiar power of mental abstraction or a peculiar mastery over attention, than upon musical training" (Helmholtz). The student should, therefore, be encouraged to believe that he can carry the following experiments to a successful issue, even if he has had no musical training. The experiments themselves may arouse an interest in music, which should then be carefully fostered by the Instructor. The psychologist who can think and imagine and remember in tones, as well as in colours and in kinæsthetic images, has a very great advantage.

The experiments have been so chosen and arranged as to familiarise the student, in order, with the essentials of qualitative work upon tone sensation. Nevertheless, it will be advisable to preface the laboratory exercises by a general lecture, in which especial attention is paid to the correlation of sound sensations with sound stimuli: the main points may be taken from Helmholtz, Pt. i. The author has found it well to make clear, at the outset, such matters as the variability of pitch-numbers, the difference between just and equal temperament, etc. Wherever it is possible, the tones and intervals referred to in lecture should be played upon some instrument. For purposes of demonstration, the Ellis Harmonical (Helmholtz, p. 17; made by Moore and Moore, 104 Bishopsgate Street, London, for about £10) has a value that can hardly be overestimated.

Preliminary Exercises. — (1) This experiment can be performed more elegantly with a Savart Wheel, a toothed wheel of wood or metal, rotating on a horizontal axis, and striking as it rotates against a piece of cardboard. With variation of speed of rotation the struck card gives a series of noises, a low tone (clang), and a high tone (clang).

(3) The determination of the predominant tone can be tested

by releasing the loud pedal, carefully pressing down the key whose tone has been selected, and repeating the stimulus. If the determination was correct, the free strings will ring out loudly in the resonance-chamber of the instrument.

(4) The introspective characterisation is extremely difficult, since the simple tone and the simple noise are ultimate sense-processes, and ultimates are never susceptible of exact definition. One cannot, therefore, expect the student to give anything but a figurative account. Thus he may say that the tone is undisturbed, uniform, clear, smooth, restful, mild, suggestive of unbroken continuance, whereas the noise is abrupt, rough, harsh, startling, unsatisfying.[1] Some of these words denote the nature of the sound itself, others indicate its affective value.

There can be no doubt that noises differ in quality or pitch. Compare the rattle of a light cart and the rumble of a heavy wagon over a paved road; the crack of a pistol and the boom of a cannon; the crash of near and the growl of remote thunder; the tinkle of a mountain stream and the pounding of a cataract. —

These exercises may be extended as far as the Instructor deems advisable. Thus the student may be set to work to classify noises, bringing together all those that belong to the same group, and arranging them in the order of intensity and of pitch, — noting differences of duration, of frequency, etc., in the complex noises. Or he may determine the tonal element in the howling of the wind, the buzz of a swarm of insects, the sound of conversation in a crowded room, etc.

Questions. — These need not all be answered at the outset, but may be given as exercises at various stages of the work.

[1] An exercise of this kind should be approached methodically and systematically. Tone and noise have, both alike, three attributes or properties: duration, intensity, quality. The enquiry should begin with the *relative* attributes, as follows:

I. (1) Do tones and noises evince a constant difference of duration?
(2) Do they evince a constant difference of intensity?

When these questions have been answered, the student passes to the *absolute* attribute, quality:

II. Are there qualitative or modal differences, in other sense-departments, analogous to the difference between tone and noise in audition?

We then get the antitheses clear, opaque, etc. — It follows that Tolstoi's fable of 'The Blind Man and the Milk' does not do full justice to psychological method.

(1) See especially Helmholtz, 23 f., 56 f.; Ellis in Helmholtz, 24 f., 57. The author prefers the term 'clang' to 'compound tone,' and 'clang-tint' to 'timbre' or 'quality.' The latter word is, indeed, quite inadmissible: the 'quality' of tone, in psychophysical language, is its pitch.

(2) Helmholtz, 310 ff.; Ellis in Helmholtz, 430 ff., 466 ff., 483 ff.

(3) Stumpf (Tonpsych., ii., 196 f.) denies the intrinsic likeness of the fundamental and its octave, and offers as test experiments the performance of a quick chromatic run, or of a glissando, upon the piano; the continuous movement of the finger down a bowed string; the continuous change of the tone of a stopped pipe. Ebbinghaus, who holds the opposite view (Psych., i., 279), objects that in these experiments the attention is so strongly drawn to the pitch-difference of the tones that the disappearance (Zurücktreten) of similarities is not surprising. But it is just because the pitch-differences are clearly brought out that the experiments are valuable: the objection begs the question. *Cf.* Stumpf's discussion in Beitr. z. Akustik u. Musikwiss. i., 1898, 45 ff.

The term 'likeness' may have various meanings. (1) Likeness may be regarded as an original and ultimate attribute or aspect, or as an 'imminent relation,' of simple qualities. Red and orange come to us with a mark of 'likeness' upon them, — more accurately, within them; no further analysis is possible. (2) Likeness may mean 'likeness of feeling-effect,' as in the former case it means likeness of direct sense-effect. Green and blue would then be like, because they put us in like moods, of restfulness or quiet. (3) Likeness may mean nearness in the scale of sensible discrimination. A given blue would be like the just noticeably different blue, because (or in the sense that) it is not easily distinguishable from this other blue. (4) Likeness may mean partial identity ('identity,' in psychology, being equivalent to 'indistinguishableness'); two colour impressions would be like which were identical, *e.g.*, in all their attributes except duration, or in all except duration and extent, etc. (5) Two simple contents may be like, in the sense that they stand in the same relation to a third (simple or complex) process.

Thus, red and orange are alike for spatial reasons: they both belong to the long-wave end of the spectrum; red and blue are alike, because they are both 'colours,' etc. — It is plain that the term 'likeness' is very ambiguous, and that it is fatally easy to slip from some one of its meanings to some other, even when we are discussing the same process or set of processes. The question of an ultimate, irreducible likeness has been keenly disputed. See Külpe, Outlines, 192; James, Psych., i., 490, 532; Mind, N. S., ii., 1893, 208; F. H. Bradley, Mind, N. S., ii., 83; K. Deffner, Zeits. f. Psych., xviii., 1898, 218; Stumpf, Tonpsych., i., 97, 111 f., 115, 425; ii., 272; etc.

The diagram referred to is Drobisch's spiral. See Ebbinghaus, 280; A. Höfler, Psychol., 1897, 99; W. Volkmann von Volkmar, Lehrb. d. Psych., i., 1884, 269, 274; M. W. Drobisch, Abh. d. kgl. sächs. Ges. d. Wiss., math.-phys. Cl. B. II., 1855, 35.

(4) Ebbinghaus, 296 f.; Helmholtz, 65 ff.

(5) See esp. Ebbinghaus, 283 ff.

(6) The most satisfactory theory is that of Helmholtz (as it finally took shape under the influence of Hensen, Exner and others), modified by Ebbinghaus. — See Ebbinghaus, 313 ff.; Helmholtz, 145 ff., 150 f., 158, 166; Stumpf, Tonpsych., ii., esp. 255 f., 450 f., 497 (see also i., refs. under *Schnecke* in index); *cf.* Beitr., i., 51 f.

EXPERIMENT V

§ 15. **The Phenomena of Interference: Beats.** — MATERIALS. — The forks required for this experiment are such as can be purchased from the music-dealers for 15¢ and 20¢. In selecting from the stock, (1) choose forks which hold their tone well, do not 'ring off' quickly (there are great differences among cheap forks in this regard), and (2) be sure that forks which bear the same pitch-mark really give a perfect unison, *i.e.*, are wholly free from beats, when you sound them together. Take both '*c*-forks' and '*a*-forks': the former give the tone c^2, the latter a^1.

Odd piano-hammers, of all sorts, can also be procured from the music-dealers.

For resonance-jars, use tall, narrow bottles (salad-dressing bottles or tall pickle-bottles answer well).

The wax should not be ordinary beeswax, which crumbles easily and is difficult to manipulate, but a mixture of beeswax and Venice turpentine, in the proportions of 3:1.

To prepare this wax, melt the beeswax and mix in the Venice turpentine. Colour (if desired) with vermilion, etc. If colouring matter is used, stir the mixture long enough to prevent its sinking to the bottom.

EXPERIMENT (1). — Most observers have a distinct preference for listening with a particular ear. It is this which is termed 'the better ear' in the text, and which should be turned towards the source of sound. See Stumpf, Tonpsychologie, i., 1883, 364.

(1), (2) For this experiment, see Helmholtz, Sensations of Tone, 1895, 161.

(3) The counting of beats is one of the fundamental laboratory arts mentioned on p. xxxv. The amount of practice necessary to accurate counting differs considerably from individual to individual. The same forks and loads should be used by several students, and the results compared. — For illustrations of actual counts, see Exp. VI.

If the forks chance to be struck unevenly, their intensities can be roughly equalised by holding the weaker nearer to, and the stronger farther from, the mouth of the resonator. But *E* should aim at equal force of strokes.

It makes no appreciable difference whether the two beating forks be held over a single resonator, or whether each be held over a bottle of its own. Indifferent tuning also answers the purposes of this experiment as well as exact, though tolerably accurate tuning gives a clearer and sharper beat.

(3)-(5). — The 5-fold repetition and the 10-sec. counting-time are arbitrary limits. With better forks, the beats may with advantage be counted (*a*) as singles, for 10 sec., (*b*) in pairs, for 20 sec., and (*c*) in fours, for 40 sec. Some observers have a natural tendency to count by two's, three's or four's: see Exp. XXXI.

(6) Beats can be counted accurately between the limits 2 and 5 in the 1 sec., and most easily when they occur 3 or 4 times per sec. Below 2, and certainly below 1, they are too slow for exact differentiation. Beats of 6 or 7 in the 1 sec. may be fol-

lowed for a few seconds by the tapping of a pencil on paper, and the dots counted; but the method is not accurate.

It is a good general principle that forks should never be touched with the unprotected hand: a warm fork flats. The small forks used in these experiments do not retain their heat long (see Exp. VI.); but, nevertheless, they should be held in sheaths of chamois leather or thick brown paper, or mounted on wooden handles. To test the effect of temperature, proceed as follows.

EXPERIMENT. — Load one of the forks with a piece of wax large enough to induce 20 or 30 beats in the 10 sec. Lay the loaded fork on the table, by the resonator. Hold the other, normal fork in the axilla for 1 minute. Now let the two forks beat. The total number of beats will probably have been reduced by 2 or 3 in the 10 sec. count. — Repeat 5 times.

A still simpler form of the experiment is to take two forks, which are in unison at the temperature of the room; to heat one of them, as described; and then to let them beat over a common resonator.

See H. Ebbinghaus, Grundzüge d. Psychologie, i., 1897, 301; Helmholtz, Sensations of Tone, 444 f.; Sanford, Course, 66 f., exps. 79, 80.

The question as to the pitch of the beating tone-complex may be divided into two questions. We may ask, first, as to the pitch of the beats themselves; and, secondly, to which of the component tones of the complex the beats are attributed. The answer to the latter question is, to some extent, included in that to the former. In neither case is the answer easy; and in neither case must the Instructor expect too close an approximation to the norm, on the part of unpractised students.

The following results were obtained (A) from an untrained observer, who was wholly 'unmusical,' and (B) from a trained observer, who was fairly musical. Stumpf's results are given for the sake of comparison.

Series I. Forks b^1 and c^2, of 480 and 512 vs. respectively. "If I give two tones, about a semitone apart, in the middle region of the scale (*e.g.*, $g^1\sharp$ and a^1 on the violin), I hear the two primary tones, but also, over and above these, a third tone which

lies between them, somewhat nearer the lower than the higher. This third tone has a very soft colouring, and with keen attention is localised within the ear; it is this tone that beats, while the primary tones remain constant. The two primary tones are, in my judgment, noticeably weakened" (Stumpf).

(*A*)	(*B*)
1. The 'fusion' does the beating, but the primary tones are heard intermittently.	1. As *A*.
2. The 'fusion' beats; the pitch seems nearer that of the lower tone. One of the primaries is heard; uncertain as to the other.	2. As Stumpf.
3. Beating tone an intermediate low tone; upper primary accompanied it. Presently changed to a higher beating tone, with lower primary as accompaniment.	3. "
4. As 3.	4. "
5. Beating tone low, with upper primary accompanying. Do not think that the beating tone is identical with the lower primary; lower?	5. Heard beating tone between the two primaries. Also heard a difference-tone (probably the tone which *A* took for the beating tone).
6, 7. As 3.	6, 7. As Stumpf.
8. As 3, except that the pitch of the higher beating tone in the second part is lower than the pitch of the upper primary.	8. "
9. A low beating tone, different from the primaries.	9. "
10. Middle tone beats; at first seemed near lower, then rose to nearer upper primary.	10. As *A*.
11. A middle tone, near lower primary, beats: both primaries heard throughout.	11. "
12. As 3.	12. "
13, 14, 15. A middle tone, nearer lower primary, beats: upper primary heard.	13, 14, 15. As Stumpf.

Series II. Attention directed to lower tone. "Without analysis, the beats are naturally apprehended as a peculiarity of the whole. With analysis, they are, in the case of the most exact hearing (Hinhören) and of adequate practice, attributed to the tones to which they really belong: and therefore, under certain circumstances [those of the present experiment], to neither of the two primaries, but to an intermediate tone. With less exact hearing, however, they are ascribed either to the two primaries or to that one of them to which the attention chances to be more especially directed. The attention then combines into a narrower whole the two moments which it is trying to grasp simultaneously, this primary tone and the beats. Many observers think that they actually hear (not merely note) the two tones themselves alternately, confusing the alternation of intensity and of attention with alternation of tone; the constant interruption of each tone renders more exact observation difficult" (Stumpf).

(*A*)	(*B*)
16. Heard middle beating tone, near lower tone, as well as the lower tone itself.	16. As *A*, except that upper primary was heard intermittently, as attention fluctuated.
17. A low beating tone only.	17. As 16.
18. Beating tone, apparently lower than lower primary; upper primary heard intermittently.	18. As 16.
19–21. A beating tone, at or near the lower primary; upper primary heard intermittently.	19–21. As *A*.

Series III. Attention directed to higher tone.

22. Low beating tone, near or at lower fork; upper primary continuous.	22. As *A*.
23–26. Middle beating tone; primaries heard intermittently.	23–26. "
27–30. Beating tone seems nearer higher fork.	27–30. "

The series show, fairly well, how near one may expect to come to Stumpf's results. In recording, the student should distinguish, so far as possible, the irregularities due to imperfect analysis and lack of practice from those due to uneven striking of the forks, fatigue, etc.

Series IV. Forks f^1 and g^1, 341 and 384 vs. "If I take tones that lie farther apart, in the same [the middle] region of the scale, I do not hear any middle tone, but only the two primaries; and these two seem themselves to beat. If, however, I concentrate the attention preponderantly upon the one of them, this always seems to be the beating tone" (Stumpf).

(*A*)

In this series, *A* always heard a beating tone which lay lower in the scale than the lower primary. This tone could be verified as a difference-tone.

(*B*)

Invariably as Stumpf. But no difficulty in hearing the beating of the difference-tone.

Series V. "If I take two tones that lie much nearer together than a musical semitone, so that they approximate the difference limen for simultaneous tones, I get one tone, and that beating. It is hard to say whether it lies between the primaries" (Stumpf).

Two forks, c^2 of 528 vs. The one fork is flatted, by means of wax, to 516 vs. Results for (*B*), as Stumpf; for (*A*), as follows.

1. Only one tone heard, nearer lower fork, and situated in space nearer the lower fork.
2. Only one tone heard, nearer lower fork.

3, 4. As 2.

5. Beating tone seemed to be nearer lower primary; but upper primary was faintly distinguished.
6. Higher tone carried the beats.
7. As 6.
8. Only one tone heard, apparently between the primaries.
9. As 8.
10. One tone beating; nearer lower primary.
11. As 10, except that when the beats grew faint the higher primary seemed to come out and take the beats.

12, 13. As 8.

14. One beating tone, near lower primary.
15. As 14.

See Stumpf, Tonpsychologie, ii., 480 ff., 490; Sanford, Course, 68, exp. 81; F. Melde, Pflüger's Archiv, lx., 1895, 623 (Melde's 'resultant' tones are not to be confused with Tyndall's 'resultant' = 'combination' tones); F. Krüger, Phil. Studien, xvi., 1900, 307, 568.

ALTERNATIVE EXPERIMENT. — The author has recommended forks for this experiment, for the reason that forks are largely employed in method-work and in research, so that it is well for the student to accustom himself to their use. Ellis, following a suggestion of Helmholtz concerning stopped organ-pipes, has devised a simple and effective instrument for the demonstration of beats, as follows. "A cheap apparatus . . . is made with two 'pitch pipes,' each consisting of an extensible stopped pipe, which has the compass of the once-accented octave and is blown as a whistle, the two being connected by a bent tube with a single mouthpiece. By carefully adjusting the length of the pipes" it is possible, first, "to produce complete destruction of the tone by interference, the sound returning immediately when the mouth of one whistle is stopped by the finger. Then, on gradually lengthening one of the pipes, the beats begin to be heard slowly, and increase in rapidity. The tone being nearly simple, the beats are well heard." — Ellis' Helmholtz, 167, *note.*

The instrument is admirable for demonstration. If connected with some sort of air-supply, it can be used for the counting-experiments of the text.

EXPERIMENT VI

§ 16. **The Pitch-difference of the Two Ears.** — MATERIALS. — The cheap forks of Exp. V. suffice for this experiment.

The *a*-forks will probably be stamped 435. This figure should be tested, preferably by the graphic method. The two forks used for the experiments quoted below were thus tested by comparison with the curve of a standard electrically-driven tuning-fork of 100 vibrations in the 1 sec. The results of five counts were: 432, 432.3, 432.2, 432.3, 432: average, 432.16 ± .128. The *g* below this is a tone of 388.94.

The *c*-forks will probably be stamped C. The *c* above the *a* of 435 is a tone of 512. Musical pitch is, however, so variable a

matter that the *c*-forks may very possibly belong to a different scale from the *a*-forks. Those used in the experiments quoted below proved, on comparison with the 100-vibr. fork, to have a pitch of $528.27 \pm .07$ (the results of four counts were: 528.2, 528.4, 528.3, 528.2). That is to say, they had been tuned to a *c* of 528, the *a* below which is an *a* of 440. — The *b* below a *c* of 528.27 is a tone of 495.25.

EXPERIMENT. — It is usually the case that the right ear is the high-pitch, the left the low-pitch ear. The pitch-difference varies in amount in normal ears. It may be as much as 1/4 of a musical tone. The average difference, for this part of the musical scale, may however be estimated at about 1/16 of a tone.

Much time may be saved in the performance of the experiment if the Instructor prepare beforehand a short series of balls of wax whose attachment to the fork produces a known number of beats. An illustration will make the method clear.

(*a*) In a series of 13 preliminary experiments, descending (from right fork 'too sharp' to right fork 'equal'), made with the *c*-forks, the Instructor found that the pitch-difference between the right (sharp) and left (flat) ears was a fraction over 4 vibrations. That is, the pitch-difference disappeared when the right fork was loaded with a bit of wax large enough to give a fraction over 4 beats per sec. when the two forks were sounded together. — In a series of 7 experiments ascending (from right fork 'too flat' to right fork 'equal'), made with the same forks, the pitch-difference found was again a fraction over 4 vibrations.

(*b*) The lump of wax which gave 4 + vibrations in the first series was divided as accurately as possible into four pieces. The value of these pieces was then determined by the beat-method. A bottle, tuned to the pitch of the loaded fork by pouring in water, served as resonator; the beats of the loaded and normal forks were counted for 5 periods of 10 sec. each. The results were as follows:

Piece 1	Beats	12	12+	12+	12	12
Pieces 1, 2	"	20+	20+	20+	20+	20+
" 1, 2, 3	"	30	30	30	30	30
" 1, 2, 3, 4	"	40	40+	40+	40	40

The *plus* of lines 1 and 4 was estimated at a quarter-beat; that of line 2 at a half-beat. The averages would therefore be: 12.1, 20.5, 30, 40.1.

For the ascending series, a new determination was made of the lump which had given 4+ beats in the second trial set, and 3 additional pieces were prepared:

Piece P	= Pieces 1, 2, 3, 4	Beats	40 , 40+, 40+, 40+, 40
Pieces P, 5	= " 1, 2, 3, 4, 5	"	44+, 45 , 44+, 45 , 45
" P, 5, 6	= " 1, 2, 3, 4, 5, 6	"	50 , 50+, 50 , 50+, 50
" P, 5, 6, 7	= " 1, 2, 3, 4, 5, 6, 7	"	60 , 60+, 60 , 60+, 60+

The *plus* of lines 1, 3 and 4 was estimated at a quarter-beat: that of line 2 at a half-beat. The averages would therefore be; 40.15, 44.8, 50.1, 60.15.

(*c*) The half-tone b^1–c^2 covers, as we have seen, 528.27 − 495.25 or 33 vibrations. The values n and n' are both a fraction over 4 vibrations. The value $n+n'/132$ is therefore $8+/132$, or approximately 1/16.

(*d*) A similar set of wax-pieces was prepared for the *a*-forks:

	Piece 1	Beats	10 , 10 , 10 , 10 , 10	Av. 10.
	Pieces 1, 2	"	15 , 14+, 15 , 14+, 15	" 14.8
	" 1, 2, 3	"	20 , 20 , 20 , 20 , 20	" 20
	" 1, 2, 3, 4	"	27+, 28 , 28 , 27+, 28	" 27.8
Piece P =	" 1, 2, 3, 4	"	28 , 28 , 28 , 27+, 28	" 27.9
Pieces P, 5 =	" 1, 2, 3, 4, 5	"	40 , 40 , 40 , 40 , 40	" 40
" P, 5, 6 =	" 1, 2, 3, 4, 5, 6	"	50 , 49+, 49+, 50 , 49+	" 49.7
" P, 5, 6, 7 =	" 1, 2, 3, 4, 5, 6, 7	"	60 , 60+, 60 , 60 , 60+	" 60.1

(*e*) The whole tone g^1–a^1 covers 432.16 − 388.94 or 43.2 vibrations. The values n and n' are 2.78 and 2.79 vibrations respectively. The fraction $n+n'/86.4$ is 5.57/86.4 or approximately 1/16.

(*f*) It is well to work out an experiment accurately, even if the result is to be merely approximate. But the Instructor must carefully distinguish, on the student's behalf, the relative accuracy of the parts of an experiment like the preceding. (1) Granted that the standard fork is accurate, the pitch-numbers of the small forks have been accurately determined within the limits given (± about 1/10 of a vibration). A longer series of determinations would have reduced the limits of variation; but the values obtained are fully adequate to the work required of them. (2) The

critical beat-values (4.01, 4.015; 2.78, 2.79 per sec.) are also sufficiently accurate. At a liberal estimate, and with all sources of error taken into account, the limits of variation could not have exceeded 1/2 a beat in the 10 sec., or 1/20 of a beat in the 1 sec. Our values are, then, at the worst, 4.01 ± .05, etc. (3) On the other hand, the final determination of equality of pitch is only approximate. A given pitch, heard by the one ear, corresponds not to a single (flatter or sharper) pitch, heard by the other, but to a small zone or band of pitches. Thus, the pitch of 528, heard by the left ear, corresponds not only to the pitch of 524, heard by the right; but to a number of flat pitches, — perhaps to any between the limits 520 and 525. Our short-cut experiment with the prepared wax balls does not tell us whereabouts in this zone our final determination falls, whether in the middle, or towards the top, or towards the bottom. Hence this determination is merely approximate.

The above sets of 8 pieces have proved sufficient for ordinary laboratory work. Their beat-values must, of course, be retested from time to time, — once a week, if they are much in use, since the turpentine evaporates readily; and *E* should always verify the beat-value of the critical load (P, or 1, 2, 3, 4). There will be occasions when he must pare off, or add on, small bits of wax.

To ensure constancy of place of attachment of the wax, a cross-line may be scratched on the fork, about 5 mm. from the extremity of the prong to be loaded. One ball is laid across this line, one on either side, and a fourth is squeezed down upon the three.

It is well to use holders for the forks, as explained in Exp. V. The experiments described above have, however, been made several times over without such holders, and in no instance have the inequalities of temperature due to unequal handling of the two forks been large enough to produce any appreciable variation in the beat-values of the loads.

LITERATURE. — Stumpf, Tonpsychologie, i., 1883, 234 f., 266; ii., 1890, 320.

Helmholtz, Sensations of Tone, 1895, 445.

Sanford, Course, 1898, 62, exp. 70 *b*.

§ 17. **Related Experiments.** (1) *The Pitch-difference in Binaural Hearing.* — We have seen that if the two *c*-forks are brought, successively, to the two ears, the right-ear fork seems to be higher pitched than the left-ear fork. But if the two forks are held *simultaneously* before the two ears, no pitch-difference is remarked: one and the same tone appears to come from either fork. Over how wide a range of pitches does this equality of simultaneous sensations extend?

Flat the left-ear fork a trifle, by sticking on a small bit of wax. Strike it and the right-ear fork, and let *O* listen to them simultaneously. Strike them *gently*, or beats will arise, and distract *O*'s attention from the required judgment of equality or inequality. — *O* still hears only one tone from the two stimuli. Add on still more wax; again, the same result is obtained. Continue the addition, until the tone from the two forks is distinctly impure, *i.e.*, until there is a distinct pitch-difference in simultaneous hearing. Be sure that this is a sensed difference, and not a physical difference betrayed by beats; on no account must the forks be struck loudly enough to beat. — Now pare off a very small piece from the lump of wax; still, perhaps, the pitch-difference remains. Pare off another morsel. The pitch-difference has disappeared again.

When this point of disappearance has been reached, the experiment is so far concluded. We have now merely to determine the pitch-number of the loaded fork. (1) Let the two forks, the loaded and the normal, beat over the bottle-resonator. Can the beats be counted accurately? Probably they are too quick. You must therefore (2) have recourse to the graphic method.[1] The difference between the pitch-numbers of the two forks will, in all probability, amount to 8–12 vibrations. Not more than 5 beats per sec. can be counted with accuracy (p. 56).

We know already, from the previous experiment, that a fork of 528 on the left is equal to a fork of 524 on the right. We must, therefore, add 4 vibrations to the 8–12 vibrations of the present determination. Equality of simultaneous sensations extends, then, over a range of 12–16 vibrations; a fork of 528, heard by

[1] See Exp. XXIV.

the left ear, is indistinguishable from a fork of 512–516, heard simultaneously by the right.

· LITERATURE. — Stumpf, Tonpsychologie, ii., 1890, 320.

(2) *The Ear as Resonator.* — (*a*) The cavity of the external ear serves as a resonance-chamber to reinforce the intensity of tones lying between e^4 and g^4.

Sound the (open) Quincke tubes,[1] g^3 to g^4, in regular order, at as even an intensity as possible. *O* will say that one or two of the highest tubes give a peculiarly cutting, piercing, screaming tone. For some subjects, the tone may be positively painful. — Repeat the experiment, in reverse order (from g^4 to g^3); and, again, in random order. Note that the piercing tones lie always within the given limits. — Now place in the ears small pieces of glass or rubber tubing, to project about 1 cm. beyond the opening of the external meatus. The character of the resonance-chamber has been changed, and the tones that were piercingly loud in the previous experiments prove to be as soft and weak as the rest of the octave.

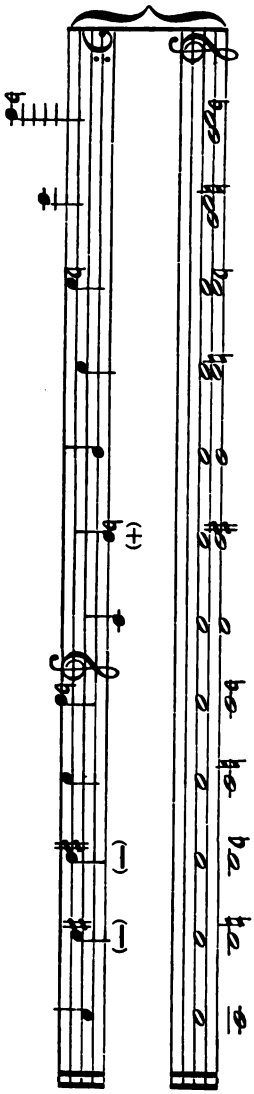
(*b*) The middle ear serves as resonance-chamber in the experience of 'singing in the ear.' The pitch of the 'singing' tone is that of the proper tone of the middle ear. It may be determined on the piano, lying usually within the limits of $f^3\sharp$ and $g^3\sharp$.

LITERATURE. — Helmholtz, Sensations of Tone, 1895, 116.
Stumpf, Tonpsychologie, i., 1883, 241; ii., 1890, 409, 443.
Külpe, Outlines, 1895, 108.

EXPERIMENT VII

§ 18. **Combination-tones.** — A. I. MATERIALS. — The pitch of the Quincke's tubes must be tested, preferably by tuning-forks and resonators, to avoid the octave-illusion. The author has known a tube to be reported as two octaves below its real pitch, when matched on the piano, and the mistake of a single octave is not at all uncommon (Stumpf, Tonpsychologie, ii., 407 f.). The pitch of the sets of tubes that are on the market seems to vary very considerably. Those of the Cornell laboratory begin with a rather sharp g^3. Another set is said to range from c^3 to c^4.

[1] See Exp. VII.



Distance of First Difference-tone below Lower Generator	Vib.-ratios of Intervals
Triple octave and major seventh	15 : 16
Triple octave	8 : 9
Double octave and major third	5 : 6
Double octave	4 : 5
Twelfth	3 : 4
Major eleventh –	32 : 45
Octave	2 : 3
Major sixth	5 : 8
Fifth	3 : 5
Major third +	9 : 16
Major second +	8 : 15
Unison	1 : 2

The pitch of an open pipe is given, approximately, by the physical formula:

$$n = \frac{v}{2l + .6w},$$

where n is the pitch number, v the velocity of sound in air (340 m. per sec.), l the length and w the width of the pipe. See Rayleigh, The Theory of Sound, ii., 1896, 202 f., 219. Recourse should be had to this formula only after the student has determined the pitch of the tubes 'by ear.'

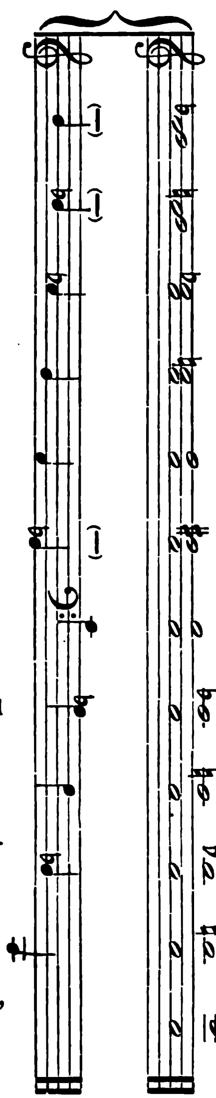
The text has assumed the correctness of the ordinary rule that the pitch of an open pipe is the same as that of a stopped pipe of half its length. The rule is only approximately correct: Rayleigh, ii., 61, 201; Helmholtz, Sensations of Tone, 88 (and Translator's note) ff. In the case of the Quincke's tubes, and of the widest intervals, the error may amount to a semitone. This fact, as well as the errors of mistuning (see p. 72 below), should be discovered by a careful *O*. Much may be done by adjustment of the blow-tubes and by regulation of the force of breathing.

PRELIMINARIES. — *E*'s adjustment of the tubes should be carefully performed and as carefully scrutinised. An unpractised subject will hardly be able to hear a difference-tone unless the generators can be sounded loudly and continuously; and, in this field of observation, practice wears off so quickly that all subjects may be regarded as unpractised, even if their attention has been called (in earlier psychological Courses, or in lectures on Physics) to the existence of combination-tones.

O should note down, in his introspective record, all the characteristics of the difference-tones heard. Thus he should remark their relative intensity, as compared with the generators; their relative difficulty of identification; their localisation, etc., etc. Beating of the generators obscures them; and those that lie near the pitch of the lower generator (roughly, those of the upper half of the octave) are difficult to hear. They are localised, not at the source of sound, but either diffusely in space, or in the ear (sometimes, if very low, in the head).

EXPERIMENT. — (1) The *lower* generating tone is kept sounding in order that *O* may be strongly impressed by its pitch, and so be able the more easily to recognise the (still lower) difference-tone.

(1)–(6) The full tale of first difference-tones, within the octave c^2–c^3, is shown in the preceding diagram. The student should prepare a similar diagram, for the octave g^3–g^4.



Distance of Second Difference-tone below Lower Generator	Vib.-ratios of Intervals
Minor second +	15 : 16
Major second +	8 : 9
Major third	5 : 6
Fourth	4 : 5
Fifth	3 : 4
Major sixth +	32 : 45
Octave	2 : 3
Octave and major third	5 : 8
Octave and fifth	3 : 5
Double octave and major second	9 : 16
Triple octave	8 : 15
[Tone absent: unison, 1 : 1, gives 1]	1 : 2

(4) In verifying *O*'s introspections, here and in following experiments, *E* should not merely sound the tube that gives the difference-tone, and be content with this single identification. When *O* has said that the tone is audible, he should give, in random order, the right tube, that of next higher, and that of next lower pitch, and require *O* to choose among them, — to say which of the three proper tones comes nearest to the heard difference-tone.

It is not probable that many of the difference-tones of this series — exp. (4) — will be heard at all.

II. The preceding diagram shows the second difference-tones for intervals within the octave c^2–c^3. The student should, again, prepare a diagram for g^3–g^4.

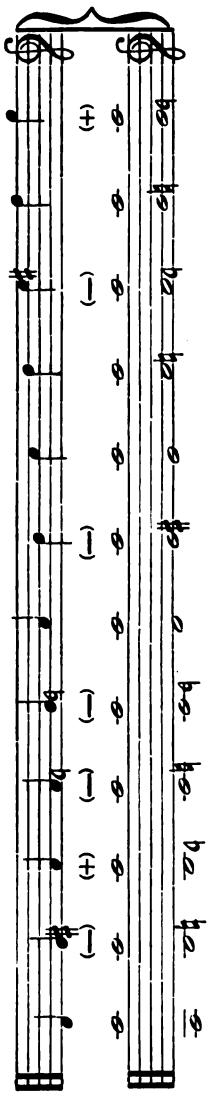
(9), (10) Probably, only the difference-tones of the intervals from fifth to octave will be audible.

III. The harmonium or harmonical is recommended, because its clangs are so constituted as to give especially strong summation-tones, and because the summation-tones are clearest with low-pitched generators which cannot be obtained from the tubes. If no harmonium is available, a few experiments may be tried with the stopped tubes (g^2–g^3). They will probably be unsuccessful.

B. I. The following diagram shows the first difference-tones for the intervals 1 : 2 to 1 : 4 (c^1–c^3). Notice that the difference-tone, which before lay always below the lower of the generating tones, now lies between the two. It can be distinguished only under exceptionally favourable conditions.

II. and III. Within the range of intervals 1 : 2 to 1 : 3 (the second and third difference-tones for which are given in the text), the second difference-tone will probably be audible over the first half, the third over the upper half. We found above, in the same way, that within the range 1 : 1 to 1 : 2, the first difference-tone can be heard over the first half, approximately, and the second over the second. It follows from this (and the inference may be tested, if time allow) that within the range 1 : 3 to 1 : 4, the second difference-tone will again be heard over the first, the third over the second half; and so on.

C. The details of this final experiment must be arranged to suit the resources of the laboratory. *O* should (if possible) be



Distance of First Difference-tone above Lower Generator	Vib.-ratios of Intervals
Major second +	15 : 32
Major third	4 : 9
Tritone –	5 : 12
Fifth	2 : 5
Major sixth	3 : 8
Major seventh –	16 : 45
Octave	1 : 3
Minor ninth –	5 : 16
Minor tenth –	3 : 10
Major tenth +	9 : 32
Octave and tritone –	4 : 15
Twelfth	1 : 4

given an opportunity of hearing the combination-tones on violin, organ and harmonium. The piano is useful for testing practice: the clangs ring off quickly, and the combination-tone must be heard at the moment the hammers strike the strings.

(1) Difference-tones are heard best with high-pitched generators, on account of their own depth; summation-tones with low-pitched generators. Difference-tones that fall below the octave C–c are, however, intrinsically so weak as to be heard with difficulty. They are plainest within the limits C–c^2.

(2) As a general rule, generating tones (or relatively simple clangs) give better difference-tones than generating clangs (or clangs rich in overtones). Tuning-forks and blown bottles (stopped organ-pipes, tubes), sounding at a moderate intensity, are therefore easier to work with than string or reed instruments. The overtones of clangs generate difference-tones of their own, which serve to distract attention from the difference-tone of the fundamentals.

In certain cases, however, the secondary difference-tones may reinforce the primary difference-tone. This happens with clangs of shrill, sharp, thin clang-tint. Try with toy trumpet, double bicycle whistle, mouth organ, concertina.

(3) The purer the interval — the nearer, *i.e.*, the generators approach to the theoretical vibration-ratio — the more distinct are the difference-tones. It must be remembered, throughout this experiment, that the Quincke's tubes are rough and cheap instruments, and that perfect tuning is not to be looked for in them.

COGNATE EXPERIMENT. — If the laboratory possess two sets of tubes, the beating of the difference-tone may be observed. Take, *e.g.*, the three stopped tubes 1, 8 and 8. If the 8's are in unison, slightly flat one of them by pouring in water. The beating (first) difference-tone g^1 will be heard.

LITERATURE. — Ebbinghaus, Grundzüge der Psychologie, i., 1897, 308 ff.

Helmholtz, Sensations of Tone, 1895, 152 ff., 529 ff.

L. Hermann, in Pflüger's Archiv, xlix., 1891, 499.

R. Koenig, Quelques expériences d'acoustique. Paris, 27 Quai d'Anjou, 1882. Pp. 87 ff.

F. Krüger, in Philos. Studien, xvi., 1900, 307, 568.

M. Meyer, in Zeits. f. Psych., xi., 1896, 177.

W. Preyer, in Wiedemann's Annalen, xxxviii., 1889, 131; Sammlung physiologischer Abhandlungen, Jena, ii., 1882, 175.

Sanford, Course, 1898, 69, exp. 82.

Stumpf, Tonpsychologie, ii., 1890, 243 ff.

EXPERIMENT VIII

§ 19. **Clang-tint.** — The coloration given to a fundamental by the upper partials of its clang is known as clang-tint in the narrow or strict sense. The other characteristics of the clang (constituents of clang-tint in the wider sense) are as follows.

(1) Most important are the *noises* which accompany the tone-complex. The clangs of all bowed instruments are mixed with a very noticeable scraping or rubbing noise. The hairs of the bow are irregular; the resin is unevenly spread; the bowing arm moves and presses irregularly. In many wind instruments we hear the whizzing or hissing of the air which breaks upon the edges of the mouth-piece. The pluck of the harp and stroke of the mandolin are characteristic.

(2) The manner in which a clang *begins*, and the rapidity with which it *dies away*, differ very considerably from instrument to instrument. Some clangs are dry, short, without ring; others are full, durable. Some set in easily, fluently; others abruptly, and yet with a certain lumberingness or sluggishness (so the clangs of the brass-wind).

(3) Pitch (compass) is a good secondary criterion. A shrill succession of notes in the highest musical octave must come from a piccolo; the tone *a*, however flutelike, cannot come from the flute.

(4) Intensity is a similar criterion. The loud tones of the brass-wind in the middle region of the scale are unmistakable.

These four criteria should be discovered by *O* and *E*, in the course of half an hour's combined introspection. Others are:

(5) Variation of pitch or intensity during the sounding of the clang. — The tremolo of the zither-string affects both intensity and quality of sensation; the oboe is characterised by the ease and wide range of its movement over the intensive scale; the organ tone within a given register is incapable of intensive fluctuation, etc., etc.

(6) A good many instruments have characteristic tasks (melodic, rhythmical, harmonic) set them, whether by the rules of their physical construction or by musical tradition. Flute and piano are characterised by certain trills and runs; the trumpet by a certain rhythmical figure; the violin by its inability to give more than two clangs with exact simultaneity; the harp by its arpeggio chords, etc., etc. All these facts assist us, if we know approximately the composition of an orchestra, to refer the various clangs to their respective instruments.

A good instance of the value of the secondary constituents in clang-tint is afforded by the following experiment. Take a stopped organ-pipe and a tuning-fork, mounted on its resonance-box, of the same pitch, say, c^2. *O* shuts his eyes; *E* sounds the two instruments, as evenly as possible, the fork by striking with a felt hammer, the pipe by blowing. So long as *O* is near enough to *E* to hear the thud of the hammer and the push of the wind in the pipe (concomitant noises), and to note the manner in which the two sounds arise (temporal differences), he has not the least difficulty in ascribing each clang to its right source. If, however, he moves a few metres away, so that the secondary criteria fail him, he readily confuses the two stimuli.

Experiment. — If *O* was impressed by the fact that the tubes of Exp. VII. gave the chromatic scale from g^3 to g^4, and knows how to apply his table of vibration-rates to the piano, he will hardly be able to perform the first step of the experiment without prejudice. In such a case, it will be better to substitute some other instrument (*e.g.*, sonometer) for the piano. The piano may then be brought in under (5). Note that the range of the flute does not as a rule extend beyond the g^3.

Again: if *O* finds the successive comparisons of (2) too difficult, — though this will rarely happen with observers who

possess even a moderately good 'musical ear,' — a simultaneous procedure may be substituted for them. Tube and whistle are sounded together, and the piston of the whistle pushed back and forth until the difference-tone is clearly heard. Then the direction in which the difference-tone deepens is determined, and the piston moved in that direction until unison is reached. The successive comparisons should always be attempted before recourse is had to the alternative method.

LITERATURE. — Helmholtz, Sensations of Tone, 1895, 19, 21, 66 ff., 118 f., 127.

Sanford, Course, 1898, 64, exp. 74.

Stumpf, Tonpsychologie, i., 235, 240, 426; ii., 406, 516–520.

EXPERIMENT IX

§ 20. **Clang Analysis: Overtones.** — *Cautions not noted in the Text.* — As a rule, the odd-numbered partials are easier to hear than the even-numbered. These latter are octaves, either of the fundamental or of some one of the upper partials that lies near it. Thus the second, fourth and eighth partials are higher octaves of the fundamental; and the sixth is the octave of the third partial. The third partial is the twelfth (octave + fifth) of the fundamental; the fifth partial lies two octaves and a major third above it; the seventh partial is the sub-minor seventh of the octave which contains the fifth.

Musically trained observers can 'imagine' the sound of the partial which is to be heard from the whole note. Unmusical persons do not know what to listen for; and it is, therefore, important that they shall hear the partial by itself (as a clang of the same tint as the clang which is to be analysed) before the full note is sounded.

As special factors, influencing the discrimination of partials, Stumpf mentions the following. (1) Observation at night-time, when the nervous disposition is more favourable. (2) Concentration of attention upon a single ear. (3) Turning of the head, alteration of the general bodily position, movement towards or away from the source of sound: influences depending upon the complicated form of the pinna, or the acoustic properties of the

room in which the experiments are made. (4) Extension of the pinna by the hollowed hand. (5) Choice of tones in the four-accented octave : see p. 66 above. — Tonpsychologie, ii., 236.

On the physics of a sounding string, see Helmholtz, 45 f. A physical demonstration may be turned to psychological account, as follows. (1) Pluck the string in the middle. The even-numbered partials are suppressed, or at least greatly weakened, while the odd-numbered sound; the clang is hollow and nasal. (2) Pluck the string at one-third of its length. The odd-numbered partials disappear, and the even-numbered remain; the clang is still thin, but better than before. (3) Pluck the string at one-seventh of its length. The first six partials are present; the clang is full and rich. — Helmholtz, 76 ff.

If the laboratory does not possess a monochord, recourse may be had to a piano or harmonium. Both instruments have upper partials of relatively high intensity. It should be noted, however, that the seventh and ninth partials are for the most part very weak, or absent, in modern pianos.

MATERIALS. — The wire of the sonometer should be thin and not too tightly stretched. If the instrument is tuned too sharp, the higher partials become difficult of recognition.

RESULTS. — Six observers, chosen without reference to musical training, heard the third partial within twenty minutes from the beginning of the experiment, and thereafter heard all the partials up to and including the seventh. Two, who had had more practice in acoustical work, reached the tenth partial without difficulty. In no case did an observer fully recognise any other partial than that to which the attention was especially directed in the experiment, though the two last mentioned 'felt' that others were present, and said that they should miss them if absent.

Helmholtz, using thin strings with loud upper partials, was "able to recognise the partials separately, up to the sixteenth." A musically trained observer, whose ear is practised in the discrimination of partials, can hear the intervals and chords formed by the lower overtones: thus it is not very difficult to hear the two tones e^1 — $\flat b^1$, when the string is sounding the *C*. Stumpf, Tonpsychologie, ii., 314; Külpe, Outlines of Psychology, 302; Ebbinghaus, Psychologie, i., 296.

Methods of Observing Partial Tones. i. *Resonators.* — A resonator is a hollow chamber (sphere, cylinder, cone) of glass or

metal, funnel-shaped at the one end for insertion into the ear, and open at the other to the surrounding air. The mass of air in the resonator, together with that in the ear-passage and the tympanic membrane itself, forms an elastic system, capable of vibrating in a peculiar manner, *i.e.*, to a particular pitch. If, then, the tone to which the resonator is tuned be sounded, the air within it is thrown into powerful sympathetic vibration, and the tone 'brays' into the ear very forcibly. Other tones, produced in the surrounding air, are considerably damped. Moreover, the masses of air in resonators have generally only very high upper partials, chiefly inharmonic with the fundamental tone, and not capable of any great reinforcement by the resonator: so that, for all practical purposes, the instrument picks out its own proper tone, and that alone, from a given mass of sound.

There are three principal forms of resonators in general use. These are the spherical resonators of Helmholtz, the cylindrical resonators of Kœnig, both made of brass, and the conical resonators of Appunn, made of thin sheet zinc. The last mentioned are cheap, and useful for most purposes; but they reinforce all the partials of their fundamental at the same time. The Kœnig resonators are made of two short cylinders, the one fitting into the other: the outer cylinder has a lid pierced with a circular opening, the inner is drawn into a funnel-shaped tube. One resonator will serve to reinforce several tones, since the inner cylinder can be drawn partly out of the other, and the contained air-space thus increased. The fundamentals to which each resonator will 'speak' are marked upon the inner cylinder, in French notation. A full set consists of 14 resonators, ranging between the limits of $G_1 = 48$ and $c^3 = 1024$ vs. ($Sol_1 = 96$ v. s. to $Ut_5 = 2048$ v. s.). The ear-tube of each resonator should be made to fit snugly in the ear-passage by means of a piece of rubber tubing slipped over the metal, or a coat of sealing-wax which is pressed into the ear while still warm and soft. It is unnecessary to plug the unused ear. — Helmholtz, 43, 372.

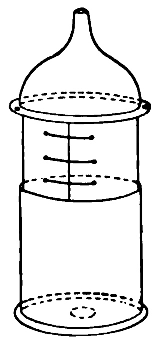


FIG. 4.—A Kœnig Resonator.

Experiments may be performed as follows. (1) Sound on the piano, harmonium or harmonical, the tone to which a resonator is tuned. Note the reinforcement of the tone when the resonator is inserted in the ear. Shift the outer cylinder (in the Kœnig resonator) slightly to and fro, until the maximum of

such reinforcement is obtained: this is necessary, since the resonator and the instrument may not be in perfect accord. Now strike the notes on either side of the resonator-tone. Note that these tones are not reinforced. (2) Lay out the resonators, in order, upon a table. Sound the note to which the largest resonator is tuned. Then test this same note with all the remaining resonators. It is best that *E* should strike the note, and hand the resonators in irregular order to *O*, who is blindfolded. *O* applies and withdraws the resonator, some few times, for 2 sec. periods, and then declares whether or not its proper tone has been heard. This procedure is necessary in the case of partials which are very weak in comparison with the tones which accompany them, and should therefore be followed through the whole experiment. The resonators which 'speak' will be found to represent the series of upper partials that attend the fundamental on the given instrument. The sonometer serves well for this experiment. (3) *O* takes a single resonator, *e.g.*, that for the c^2. *E* plays successively a number of notes that are lower in the scale than the c^2, and *O* declares whether or not the resonator 'speaks' to them. The resonator tone will be heard whenever a note is struck which contains the c^2 as an upper partial: it will be heard, *e.g.*, from the c^1, *f*, *c*, *A*♭, *F*, *D*, *C*.

It should be noted that the hearing-out of overtones by means of resonators is not an exercise in psychological analysis of the same kind as their discrimination by the unaided ear. The tonal material under investigation is changed by the introduction of the resonator; some one of the partial tones is intensified, while the remainder are damped. Nevertheless, work with resonators forms good preliminary practice for the experiment of the text.

ii. *Sympathetic Vibrations of Strings.*—(4) Press down the *c*-key of the piano, slowly and gently, so that the hammer does not strike; and hold it down, so that the *c*-strings are free to vibrate. Now strike the *C*-key; let the note sound out loudly, and after 2 sec. damp it by releasing the key. The *c* is distinctly heard. Repeat the experiment with a number of keys chosen between the *C* and the c^2. The *g*, c^1, e^1, g^1 and c^2 may all be heard, though in decreasing intensity. (5) Press down the c^1-key, as the *c*-key was pressed in the previous experi-

ment. Play in succession (and immediately damp) the notes c, F, C, $A_1\flat$, F_1, D_1, C_1; c^2, g^2, c^3. Note that the c^1 sounds in every case.

On the mechanical proof of sympathetic vibration of the piano strings, see Helmholtz, 47.

iii. *Beats.* — (6) This experiment is best performed on the harmonium or harmonical. "Keep down the note C, and touch in succession the notes c, g, c^1, e^1, g^1, etc.; but in touching the latter press the finger-key such a little way down that the note is only just audible. This slightly flattens each note, and slow beats can be produced" (Ellis) between the partial contained in the C and the flattened note of the reed whose fundamental is the partial in question. Verify by sounding other notes than those of the upper C-partials, and observing that the beats (when present) are much quicker. — Helmholtz, 22.

iv. *Direct Analysis by the Ear.* — This is the method explained in the text. A number of subsidiary experiments may be added here. (7) "To the objection which is sometimes made that the observer only imagines he hears the partial tone in the compound, because he has just heard it by itself," it may be replied "that if e^2 is first heard as a partial tone of c on a good piano, tuned in equal temperament, and then e^2 is struck on the instrument itself, it is quite easy to perceive that the latter is a little sharper. This follows from the method of tuning. [The partials are not only heard as simple tones; they are also heard always in just temperament. The e^2 as overtone has a pitch number of 660; the e^2 of the piano a pitch number of 665.3.] But if there is a difference of pitch between the two tones, one is certainly not a continuation of the mental effect produced by the other" (Helmholtz, 50). (8) The upper partials contained in the human voice may be heard as follows. "Let a powerful bass voice sing $e\flat$ to the vowel O in *sore;* gently touch $b^1\flat$ on the piano, . . . and let its sound die away while you are listening to it attentively. The note $b^1\flat$ on the piano will appear really not to die away, but to keep on sounding, even when the string is damped by removing the finger from the digital, because the ear unconsciously passes from the

tone of the piano to the partial tone of the same pitch produced by the singer, and takes the latter for a continuation of the former. But when the finger is removed from the key, and the damper has fallen, it is of course impossible that the tone of the string should have continued sounding. To make the experiment for g^2, . . . the voice should sing to the vowel A in *father*" (Helmholtz, 51). (9) If a tuning-fork tone is allowed to die away, the pitch of the tone seems slowly to rise; the overtones disappear more slowly than the fundamental. If a low note on the piano keyboard is struck, and the key held down while the tone dies away, the upper partials ring out, in irregular order, as the tone weakens. Something similar may be observed on the sonometer, and on the harmonium, if a low reed is sounded and the air allowed gradually to escape from the bellows. — Stumpf, i., 242; ii., 237. (10) Clamp down a low key on the harmonical keyboard, and keep the note sounding for some time, — Mach speaks of half an hour, but ten minutes will probably suffice. Different partials ring out successively from the mass of sound (E. Mach, Grundlinien der Lehre von den Bewegungsempfindungen, 1875, 58; Analyse der Empfindungen, 1886, 127).

Literature. — Helmholtz, Sensations of Tone, 36–65; Stumpf, Tonpsychologie, ii., 231–243; Sanford, Course, 73 ff., Exps. 86–89.

Acoustic Instruments. — The laboratory should possess a set of Kœnig forks (12 forks, Fr. 485); a set of resonators (14, Fr. 380); an Ellis harmonical (see p. 52 above); an Appunn tonometer (Mk. 350) with bellows table (Mk. 120); and a Kœnig sonometer (Fr. 112).

CHAPTER III

Cutaneous Sensation

§ 21. **Cutaneous Sensation.** — Investigation of the cutaneous sensations has moved so rapidly during the past five years that there is no adequate account of them to be found in the text-books. Külpe's sections (Outlines of Psychology, 1895, 87, 92) are already out of date. The experiments that follow emphasise the principal points in the sense psychology of the skin; but they should be supplemented by lectures on the basis of the monograph literature.

The student may presently be asked to analyse the perceptions of impact, resistance, etc. See Titchener, Outline of Psych., 1899, 64 f.; Helmholtz, Sensations of Tone, 1895, 63.

Preliminary Exercises. — The temperature experiment was suggested by John Locke (1632–1704). See An Essay concerning Human Understanding, Bk. ii., ch. 8, § 21.

A pretty variation of the pressure experiment is this. Get a number of similar corks, and cover the lower surfaces with substances of varying roughness: plush, velvet, flannel, buckram, perforated tin (overlaid with tissue paper, to avoid the temperature effect), hard and soft wood, etc., etc. Set the corks down gently and evenly upon the skin, and let *O* try to identify or describe the pressing surfaces. The experiment brings out our extreme dependence upon active pressure (touch), as well as the fact of adaptation.

Questions. — (1) See Hering, in Hermann's Hdbch. d. Physiol., iii., 2, 1880, 419 ff.; Külpe, Outlines, 95. The materials for criticism are furnished by Exp. X.

(2) In all probability, the free nerve endings of the epidermis are the pain organs; Krause's end-bulbs, the organs of cold; Ruffini's cylinders, the organs of warmth; and the hair-bulbs and Meissner's corpuscles, the organs of pressure. See M. von Frey, Ber. d. kgl. sächs. Ges. d. Wiss., March 4, 1895, 180 ff.

On cutaneous sensations, see Wundt, Phys. Psych., i., 1893, 410; O. Funke and E. Hering, in Hermann's Hdbch. d. Physiol., iii., 2, 1880, 289, 415; Stout, Manual, 186; Titchener, Outline, 63, 73; A. Goldscheider, Gesammelte Abhandlungen, i., 1898; Foster, Text-book of Physiol., iv., 1891, 1412. The experimental literature begins with the monograph of E. H. Weber, Der Tastsinn und das Gemeingefühl, published in R. Wagner's Handwörterbuch d. Physiol., iii., 2, 1846, 481.

EXPERIMENT X

§ 22. **Temperature Spots.** *Cautions not noted in the Text.* — If the students are entirely ignorant of the nature of isolated temperature sensations, it will be well to preface the experiment by a few rough trials. A blunt pencil-point drawn slowly over the back of the hand will give rise to several flashes of cold. The warm spots are more difficult of identification than the cold spots: trials may be made with the heated cylinder upon the eyelids. Care must be taken that there is no scratching of the skin by the point of the cylinder. Where necessary, the point should be rubbed gently upon fine emery paper until it is sufficiently rounded. The dyes are a little difficult to manage, at first. The solution soon dries on the brush, and a blot of colour may be made where the dot is wanted. The solution must be kept quite weak, and the brushes frequently dipped in water. Sometimes, again, the nitrate of silver in the indelible ink sets up an inflammation of the skin. If this is at all troublesome, the margins of the marked area may be left unexplored. If the skin proves to be extremely sensitive, the four corners of the area may be marked by dots, and the side-lines drawn in dye (brown, *e.g.*). More care is then needed to ensure accurate localisation of the temperature spots in the two maps.

No attempt must be made, during a single sitting, to verify the spots once found; fatigue is inevitable, and confusion and self-distrust result. Errors are apt to be made in localisation, by the fact that *approach* to a cold or warm spot will set up a weakly cold or warm sensation. Even after repeated cautions, a student is likely to enter this approximation-sensation in his maps rather than the more intensive sensation which he can

obtain by moving the cylinder a little farther, until it lies exactly over the temperature spot. The Instructor will then be called upon to decide whether two spots, lying close together in the two preliminary maps, are to be entered in the third, final map as one or two. The question can generally be settled by noticing the relative positions of the spots. Thus, when the area has been worked over in the RL and PC directions, the spots, if really one, will lie too far to the R and too far P, respectively: should the PC spot lie C of the RL spot, or the RL spot lie to the L of the PC spot, the presumption is that the marks belong to two distinct sense-organs. The general accuracy of the student's work must also be taken into account. In cases of doubt, the Instructor himself, not *E*, should make a special test to determine the matter. The best rate of movement and degree of pressure vary somewhat from individual to individual. They must be learned by practice.

Practice, indeed, on the part both of *E* and of *O*, is the chief condition of successful work in this experiment. It is hardly possible, in a drill Course, to give the student time enough for really thorough practice. Hence if, say, two-thirds of the spots first found are verified at the second sitting, and if this sitting leads to the discovery of a fair number of new spots, the Instructor may be well satisfied. The main thing is to convince the student that the spots are not artifacts, products of imagination, and to afford him opportunity for introspection.

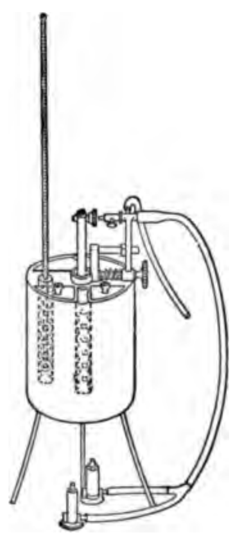


FIG. 5.

For the temperatures to be employed if this experiment is more accurately performed, see Pt. i., p. 57. The Instructor will find, however, that all the introspective points raised in the text can be brought out by the rough method here recommended. Even as it is, the experiment is likely to run to undue length; and work with constant temperatures demands a proportionately much longer time.

Fig. 5 shows a device for keeping water at a constant warmth. A copper vessel is fitted with a Roux regulator and thermometer, and heated by Friedburg burners connected with the gas supply. The

temperature will remain constant to .1°, within the limits and for the time required.

A good instrument of the Blix pattern (mixed warm and cold water flowing in a pointed tube) is made after J. McK. Cattell's design by the mechanician of the Columbia Univ. Laboratory. Instruments can also be obtained in which a platinum point is warmed by electrical means.

QUESTIONS. — *E* (1) The cold spots are the more numerous. The type of arrangement of both spots is the same. We find (*a*) groups or clusters of spots, — small areas of temperature, as it were. These are commoner for cold spots than for warm. In the case of cold, though never in that of warm, these areas are sometimes unanalysable into discrete spots. We find (*b*) curves or chains of spots. These are sometimes of the same quality throughout, sometimes of intermingled cold and warm spots. Often they enclose small, irregularly-shaped, insensitive areas. (*c*) We find isolated spots.

The dots for the warm spots should be larger, since the area of radiation is sensibly greater in the case of the warm sensation than it is in that of the cold.

E (2) The intensive. — Emphasise the fact that the spots are not all equally sensitive, but are 'tuned' or adjusted to respond to a given stimulation by sensations of varying intensity. Verify it by experiments made at known spots, if there is any doubt of it in the student's mind.

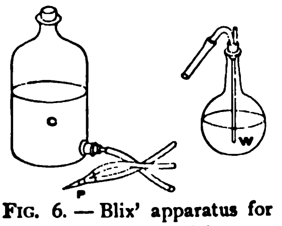


FIG. 6. — Blix' apparatus for the investigation of the temperature sense. C = cold, W = warm water; P = metal point.

O (3) The cold sensation is localised more superficially than the warm. The cold is more restricted, less extended, than the warm. The cold seems to lance down, from above; the warm seems, oftentimes, to well up, from beneath. The cold is set up at once, in a moment; the warm comes gradually to its full intensive development. The cold is continuous, all of a piece, a solid point of cold; the warm is at times discontinuous, bubbly or fizzling, a number of spurts of warmth.

O (4) It is probable that pressure was involved in every case.

The instrument used is not delicate enough to allow of complete isolation of the temperature spots.

E and *O* (5) It would be well to determine (1) the nature and number of the spots over the corresponding area of the right hand. The distribution of the spots might also (2) be tested at different parts of the body. Are the more peripheral parts, *e.g.*, more or less richly endowed with temperature organs than the trunk? Do dorsal and volar surfaces differ? How does the median line of the body compare with the lateral parts? These and similar problems may be worked out by the student, as time permits.

RELATED EXPERIMENTS. (1) *Mechanical Stimulation of Temperature Spots.* — Localise, by aid of one of the preliminary maps, an intensive cold spot. Pull the skin taut by finger and thumb. Tap it, at first lightly and then by degrees more and more strongly, with the pressure spot apparatus of Exp. XII. If you find the right intensity of tapping, the sensation of cold will flash out, as it does in response to the cooled cylinder. — Perform the same experiment with a warm spot. The same result can be obtained, though with greater difficulty.

(2) *Analgesia of the Temperature Spots.* — Localise an intensive cold spot. Soften the skin by rubbing it with soapy water. Stretch it taut. Thrust the point of a very fine needle, previously warmed between finger and thumb, down into the spot. You will find, probably in four or five cases out of ten, that you get a sensation of cold entirely free from the pricking pain which the stimulus sets up at neighbouring parts of the skin. The cold spots are analgesic; but as often as not, — perhaps rather more often than not, — you will strike a pain spot at the same time that you hit the temperature spot. Perform the same experiment, with a cooled needle, at a well-defined warm spot.

(3) *Inadequate Thermal Stimulation.* — The *O* of the experiment described in the text may have noticed (*a*) that sometimes, when cold spots were being sought, but the cooled cylinder had been too long in use, had been inadvertently held in the warm fingers, or what not, a *warm* sensation welled up under its touch; (*b*) that sometimes, when warm spots were being sought, and the

heated cylinder was a little over-hot, a *cold* sensation flashed out; and (*c*) that sometimes, when warm spots were being sought, but the heated cylinder had been too long in use, a *cold* sensation was produced. The first and third of these experiences are the result of carelessness. The second, however, is extremely interesting, and raises the general question whether we can make a cold spot respond by cold to a warm stimulus, and a warm spot respond by warm to a cold stimulus.

The facts seem to be as follows. (*a*) Normally, the warm spot replies only to the warm stimulus, by a sensation of warmth; the cold spot replies only to the cold stimulus, by a sensation of cold. (*b*) There is, however, a 'paradoxical' cold sensation (von Frey). Localise an intensive cold spot, and stimulate it by a metal point heated to 45° C. or over. It responds by an instantaneous sharply defined cold sensation. At certain parts of the body, parts which have a highly developed cold sense and little sensitivity to warmth, the paradoxical cold sensations form a serious obstacle to the mapping of the warm spots. Oftentimes the sensation is obtained most clearly not from the skin directly over the marked spot, but from points lying immediately adjacent to the mark. No explanation is at present possible. *Cf.*, however, the momentary chill experienced on plunging into a hot bath. (*c*) There is no paradoxical warm sensation. Or, at least, no such sensation is mentioned in the literature; and the author, in a very large number of experiments, has invariably failed to evoke a warm sensation from a warm spot by the application of a cold point. (*d*) A warm spot never responds to the cold cylinder by a cold sensation. Kiesow has worked with temperatures as low as —6° C., with this result; and the author's experiments fully bear it out. (*e*) A cold spot never responds to the heated cylinder by warmth. Kiesow's statement that he has hardly ever found a cold spot that would not reply to stimulation of 47°–50° C. by a sensation of warmth is a misreading of the facts. In reality, radiation occurred, and the process observed was not that of warmth, but of heat (see Exp. XI., p. 90).

LITERATURE. — The temperature spots were discovered independently by M. Blix (Upsala Läkareförenings Förhandlingar,

1883; Zeits. f. Biologie, XX., 1884, 140); H. H. Donaldson (Mind, O. S., x., 1885, 399); and A. Goldscheider (Arch. f. [Anat. u.] Physiol., Suppl., 1885). Goldscheider's paper is the longest and most systematic: it is published in his Gesammelte Abhandlungen, i., 1898, 107; *cf.* also pp. 53, 94, 100, 275, 301.

Reference may also be made to papers by F. Kiesow (Philos. Studien, xi., 1885, 135; xiv., 1898, 589) and J. F. Crawford (Psych. Rev., v., 1898, 63). For the paradoxical cold sensation, see M. von Frey, Ber. d. math.-phys. Classe d. kgl. sächs. Ges. d. Wissensch. zu Leipzig, March 4, 1895, 172.

EXPERIMENT XI

§ 23. **Temperature Sensitivity: Areal Stimulation.** *Cautions not noted in the Text.*—The student should be made clearly to understand that the previous experiment aimed to determine the number and nature of the temperature organs lying within a given area, whereas the present experiment seeks to determine the sensitivity (manner of functioning) of the temperature sense over a given area.

It will be well to have a few preliminary trials made, say, on the palm of the hand, in order to accustom the student to the meaning of the three categories 'intensely cold,' 'cold,' 'just cold,' etc. 'Cold' means distinctly, unmistakably, definitely cold; 'intensely cold' means surprisingly, unusually, even unpleasantly cold; 'just cold' means 'cold, if anything,' 'perhaps a little cool.' 'Warm' means, in the same way, unmistakably, clearly warm; 'just warm' means 'warm, if anything,' 'perhaps a little lukewarm'; 'intensely warm' means glowingly, impressively, or surprisingly warm. Note that the perception of heat cannot be obtained from a stimulus of the temperature employed (*cf.* Related Exp., below). A very little practice will suffice to render introspection accurate.

If possible, the experiment should be performed three times over, at intervals of about a week. The results of the first performance are likely to be unreliable, from the fact that the student does not notice differences of sensitivity within the area of the stimulus, until his attention has been called to their possibility by questioning.

Specimen of Results. — Fig. 7 shows two 'cold' maps, taken from a wholly unpractised but attentive *O*, at an interval of a week. Four degrees of cold are here recorded: 4, very intensely cold; 3, strongly cold; 2, moderately cold; 1, weakly or just barely cold. It will be seen that only in one case is there any material discrepancy between the introspections. It is, however, better to reduce the four degrees to three.

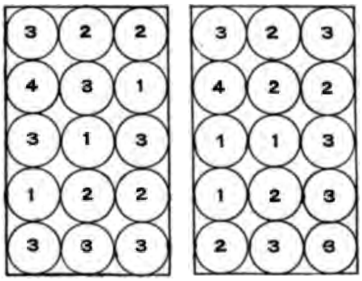


FIG. 7.

Fig. 8 is a final 'cold' map. The black areas are those of very intense cold; the lined areas those of strong cold; the dotted areas those of moderate cold; and the white areas those of weak or just noticeable coolness. The value of such a map depends, of course, upon the constancy with which the areas reappear in subsequent tests. For an attentive and honest *O*, this constancy is practically absolute.

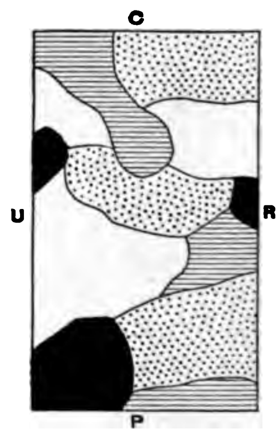


FIG. 8. — *P*, peripheral; *C*, central; *U*, ulnar; *R*, radial.

QUESTIONS. — *O* (1) It is a general rule that the introspection of sensation intensities is more difficult than that of sensation qualities. The former is a comparative or relative introspection: the intensity is always a 'stronger' or a 'weaker'; the latter is an absolute introspection: a quality is a 'this' or a 'that.' In accordance with this rule, the present experiment should be the more difficult. More especially would this be the case, if *O* had found the differences within the stimulus circle referred to in Question (3).

On the other hand, the degrees of intensity, introspection of which is here called for, are so chosen as to render the introspection as easy as possible. Moreover, the student is not accus-

tomed to the determination of punctiform sensation qualities; and so may have found the preceding experiment difficult. Hence, if the differences of Question (3) have not been remarked, the present experiment may be given as the easier.

The 'reasons for the answer' are, obviously, of much greater psychological importance than the answer itself.

E (2) Yes. Sensitivity to cold has a greater range, intensively and extensively, than sensitivity to warmth. Note the fact that high intensity values are commoner for cold, and that insensitive areas occur more frequently and in greater extent when the skin is being tested for warmth. Further: if the two final maps be laid over one another, or viewed in a stereoscope, it will be found that the areas sensitive to warmth and cold partially overlap (*cf.* the mixed chains of the last experiment). And certain irregularly-shaped areas will be found to show insensitivity to both forms of stimulus.

O (3) Oftentimes there are differences, though their introspective demarcation is not easy. Occasionally, however, a region of best sensitivity borders directly upon a region of insensitivity.

E and *O* (4) This question is best answered by the counter-question: *Is* the skin, under the conditions of stimulation found in everyday life, a mosaic of sensitive and insensitive parts? Is it not rather a continuously sensitive organ?

It is true that, under experimental conditions of extreme refinement, two adjacent cold or warm spots will give rise, when simultaneously stimulated, to two distinct sensations. But in ordinary life the sense organs do not receive such delicate stimulation. For the most part, temperature stimuli come from extended surfaces. Now we have found, in our own experimenting, that there is an 'approximation sensation' of temperature; the neighbourhood of the sensitive spot is itself sensitive; the sensation radiates from the end-organ over a certain area of the surrounding skin. We must suppose, then, that areal stimulation really calls forth an area (not a mosaic) of sensation. We might, perhaps, expect to find intensive differences within this area, since the approximation sensation is weaker than the spot sensation; but the cognition of small intensive differences is difficult, and these may well be subliminal. — The retinal

mosaic of rods and cones would thus afford a good analogy. *Cf.* also the mosaic of pressure spots.

There is one fact, easily verified by experiment, that calls for special explanation in this connection. It is this: if the stimulated skin area have a very few intensive spots and a greater number of weakly sensitive spots upon it, it is always regarded by *O* as an area of intense sensitivity; the few good spots give a character to the whole area. We must suppose, in this case, that the weak sensations, though they do not come to consciousness as special temperature sensations, nevertheless form the basis of *O*'s judgment of continuity. They give the attribute of area (unbroken continuity) to the total temperature sensation, just as the few intensive sensations give the total sensation its intensity. —

If we are not satisfied with this 'summation' theory, we may have recourse to a subsidiary hypothesis. We may suppose that the gaps in sensation are filled out by association. We see, *i.e.*, that the stimulus presents an unbroken surface; and we carry over this continuity, by visual association, to the skin. *Cf.* the filling-out of the blind spot of the retina.

RELATED EXPERIMENTS. — *The Perception of Heat.* — We have seen that the cold spots respond by a sensation of cold to intensive inadequate thermal stimulation (stimulation by a warmed point of 45°–50° C.). What happens when the skin is subjected to *areal* stimulation of a temperature of 45° C. or over, — *i.e.*, when the warm spots of the area give warmth, and the cold spots cold, in sensation? How do the two temperature qualities mix? It responds by a new temperature quality: the quality of *heat.* Heat is a fusion or mixture of warm and cold.

The student must first assure himself of the existence and nature of the hot perception. It can be obtained from the skin of the arm over the elbow joint (volar surface), at a temperature of 45°–48° C.; on the forehead (upper portion, centre), at 48°–50° C.; and on the mamilla at as low a temperature as 40° or 41° C. It must be carefully distinguished, in introspection, from the sensations of warmth and of pain. It differs from both in quality. It may further be distinguished from warmth

by the facts (1) that it is less diffuse, more concentrated, and (2) that it is localised more deeply.

To prove that the heat perception is really a fusion of cold and warm sensations, the following tests may be made. (*a*) Find a place upon the skin which has cold but not warm spots. Here, nothing but cold and pain are obtainable from stimulation with high temperatures. (*b*) Find a patch of skin that has warm but not cold spots. Here, nothing but the familiar, diffuse warmth can be obtained, until the temperature sensation passes over into pain. (*c*) Stimulate a place that has poor warm sense, and good cold sense (*e.g.*, the upper portion of the forehead, in the neighbourhood of the median line) by temperatures ranging, at half-degree intervals, from 40°–52° C. Up to about 48° you get merely the faint warmth that comes from stimulation of the poorly sensitive warm-spots. From this point, *i.e.*, from the point at which the cold spots would give the paradoxical cold sensation, the dull warmth changes to heat. There is no pain; nothing but a change of temperature quality. The introspective records will probably speak of a 'spear point' of heat, or of a 'throb of heat beneath the skin.'

This experiment serves to emphasise the incongruity between the physical (stimulus) and the mental (sensation) in the sphere of temperature. We are apt to think of temperatures physically, as *degrees* of one and the same quality (thermometer scale). Warmth and cold are, psychologically, qualities of different senses, proceeding from different sense-organs. If they differed merely in degree, they would cancel each other when mixed, as positive and negative numbers cancel each other when summed; they could not possibly fuse together, to produce a third conscious quality. Heat (warmth × cold) may be compared, psychologically, to colour (colour proper × brightness), or taste (taste proper × smell), or the note of a musical instrument (fusion of a number of tones and noise). All alike are illustrations of 'fusion.'

LITERATURE. — Goldscheider, *op. cit.*; for the quality of heat, S. Alrutz (Upsala Läkareförenings Förhandlingar, 1897; Skand. Arch. f. Physiol., vii., 1897, 321; Mind, N. S., vi., 1897, 445; vii., 1898, 141).

EXPERIMENT XII

§ 24. **Pressure Spots.** *Cautions not noted in the Text.* — It is well to familiarise the student beforehand with the pressure quality. Let him close his eyes; then let the point be set down several times on the back of his hand, in the near neighbourhood of an isolated hair. If the point is set down at all intensively, there will probably be a dull, diffuse, contentless pressure sensed at every application: this is due to the extension of the deformation of the skin to neighbouring pressure spots, and their consequent weak stimulation. At one place, to windward of the root of the hair, however, the true pressure quality will be obtained: a distinct, sharply localised sensation, of the kind that one might imagine to be set up by the resistance of a hard seed embedded in the cutis and now forced inwards by the pressing point. If the pressure be lighter, the pressure quality is delicate, a little ticklish, of a kind to hold the attention very easily.

The approximation-sensation must be guarded against: *cf.* temperature. Less practice is required for the identification of pressure spots than is needed in temperature work.

QUESTIONS. — *E* (1) Practically every hair has its pressure spot, which lies to windward of the hair itself. If the hair is dark, so that its course can be followed beneath the skin, it will be seen that the pressure spot lies directly above the hair-bulb.

Yes. Movement of the tip of the hair calls forth a weak pressure sensation, often somewhat ticklish in character.

Yes. A good way to prove this is to work from the back (hairy) of one of the finger phalanges down over the side (hairless) of the finger. The pressure spots will be found to be about equally distributed in the two places.

The sensations are indistinguishable.

E (2) The intensive. Emphasise the fact of 'tuning,' for the pressure spots, as before for the temperature spots. An intensity of pressure which evokes the 'seed' sensation from one spot may evoke only the weak, delicate pressure sensation from a neighbouring spot.

O (3) See above, under 'Cautions.'

O (4) Cold. As regards both time and space attributes, there is more likeness between pressure and cold.

O (5) Probably tickling and cold (mechanical stimulation of cold spot). Such sensations, if they occurred, should, of course, have been noted by *O* in the course of the experiments. The question is given here merely as a check upon *O*'s accuracy.

E and *O* (6) It would be worth while to ascertain the distribution of the spots at various parts of the body (*cf.* the finger tips with the upper arm, *e.g.*); to ask whether there is any func-

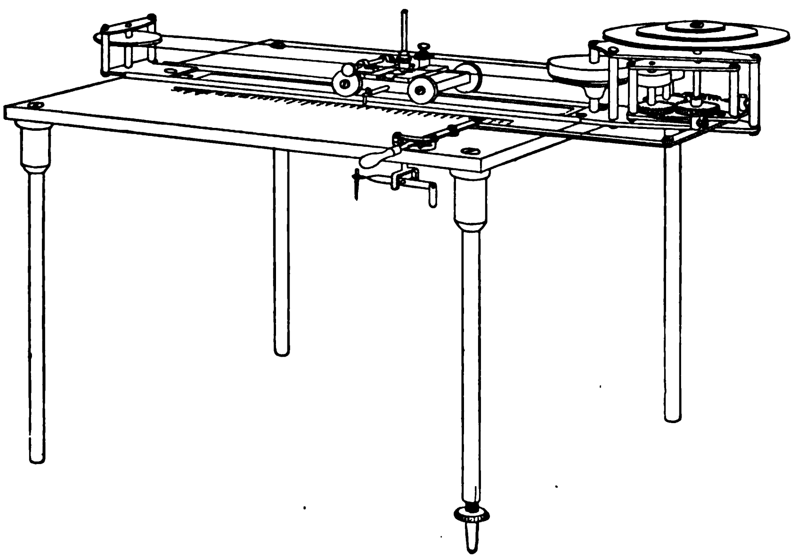


FIG. 9. — Kinesimeter.

tional difference between the hairy and the hairless parts of the skin, — whether the pressure sensations yielded by the hair-organs and by the organs scattered over the hairless parts of the skin are put to precisely the same use by the organism; to note, and enquire into, the apparently inverse ratio of temperature to pressure organs (palm of hand, used for grasping: good pressure, poor temperature sensitivity; back of hand: better temperature, worse pressure sensitivity); to experiment upon areal sensitivity to pressure; etc. Here, again, a whole list of problems presents itself, to be worked out as time permits.

LITERATURE. — A. Goldscheider, Arch. f. [Anat. u.] Physiol., Suppl., 1885 (see Gesam. Abh., i., 1898, 185, etc.); M. von Frey, Ber., etc., Dec. 3, 1894, 293; Aug. 2, 1897, 462; Abh. d. math.-phys. Classe d. kgl. sächs. Ges. d. Wissensch., xxiii., 3, 1896, 175; M. von Frey and F. Kiesow, Zeits. f. Psych., xx., 1899, 126.

INSTRUMENTS. — Fig. 9 shows the kinesimeter of G. S. Hall and H. H. Donaldson (Mind, O. S., x., 1885, 403, 557), in improved form (E. W. Scripture and E. B. Titchener, Amer. Journal of Psych., vi., 1894, 425; vii., 1895, 150). $100.

EXPERIMENT XIII

§ 25. **Pain Spots.** — The two 20 sq. mm. areas are recommended, in order that the students may have an opportunity of comparing the distribution of pain spots with that of the pressure spots, as determined in Exp. XII. It is advisable, if the pain spots are to be stimulated in complete isolation from pressure spots, to work upon a portion of the skin which is hairy (so that the pressure spots can be easily identified), but shows fairly large hairless interspaces. Good areas can be found upon the outer (dorsal) surface of the upper arm: but sleeves render it difficult to work upon this part of the body. In any case, the area chosen must be quite small, or thorough exploration is impossible.

The method given in the text is, probably, the most convenient. It is possible to work with dyes, as in the preceding experiments, and to transfer the skin map to architects' paper. The dye is, however, apt to run upon the moistened epidermis; and even if the area is subdivided, and the one half kept moist while the other half is under stimulation, there will be times when the wet point must approach a marked spot so closely as to set a fringe of dye spreading over the skin. Moreover, the dot of dye will, at the best, be too large for the pain spot. The author has, therefore, given up this method for that of mapping by means of the skin-furrows.

The horse-hairs may be replaced by a pointed hog's bristle, or by a fine sewing needle, sharpened still further upon an oil-

stone. The objection to the former is that, after a few applications, it has an obstinate tendency to bend, and that it blunts easily. The objection to the latter is that it readily pierces the epidermis. Horse hairs seem to wear better than bristles, though the student will do well to have at least half-a-dozen, ready pointed, before the experiment begins. For accurate work, they should be standardised by von Frey's procedure: Ber., etc., July 2, 1894, 185 ff.

Questions. — (1) Pressure, though pain is every whit as distinct from pressure as pressure is from cold (p. 93 above).

(2) This question may be answered by extracts from a laboratory note-book. "Each sensation of *cold* was as clear as crystal. Each was so distinctly one-of-its-kind as to allow no doubt on the part of *O* as to its identity. There were, however, different degrees in the sensations of cold. . . . The sensations of *warmth* are harder to distinguish at first than those of cold. But with practice one readily distinguishes them. The feeling is just one of warmness — not heat — and not at all lively. . . . The *pressure* sensation is like an electric-battery shock reduced to small proportions. It was very lively. It was quick and thrill-like, and seemed to leave an after-image. . . . The sensations of *pain* are very different from either the temperature or the pressure sensations. They are minutely fine, wirelike, thin; much livelier and more thrill-like than the pressure sensations. I could almost posit an area for the pressure sensations, but the pain sensations seemed to have no bigness at all."

(3) The pain spots are more numerous than any of the others. There is no outward indication of their existence. See von Frey, Ber., etc., Dec. 3, 1894, 289.

(4) The moistening lowers the pain limen. *Cf.* the sensitiveness of the scalp when the hair is brushed after bathing.

(5) We might determine the number and distribution of the spots over the corresponding area of the other side of the body; compare the number and 'tuning' of the spots at more and less exposed parts of the body; ascertain whether the number of pain spots in a given area varies proportionately or inversely as the number of pressure or temperature spots; experiment upon areal sensitivity to pain, etc.

Specimen of Results. — The accompanying five maps were made from a circle of 2.5 cm. diameter upon the dorsal surface of the left upper arm. Special precautions were taken to keep the circle absolutely constant from day to day. No. i., the first

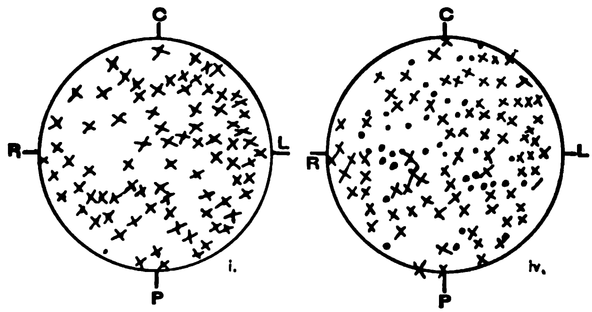


FIG. 10. — Hairs and pressure spots. The hair-crosses do not correspond exactly, owing to the stretching of the skin.

map drawn, shows the hairs. Nos. ii. and iii. show the cold and warm spots respectively. No. iv. gives the pressure spots: the crosses here and in i. indicate coincidences of hair and pressure spot. Finally, no. v. shows the pain spots. These are, without any doubt, too few. The experiments were performed with

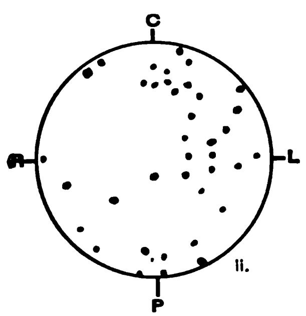


FIG. 10. — Cold spots.

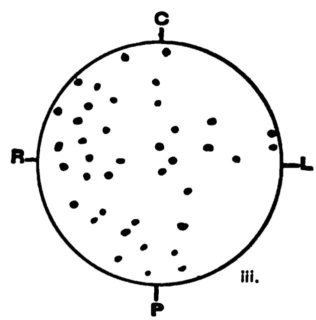


FIG. 10. — Warm spots.

a hog's bristle, which does not allow of complete isolation of the spots (von Frey, Abh., etc., 244); and it was necessary to hurry the latter part of the investigation, with the result that the area became somewhat sore and irritable.

FURTHER EXPERIMENTS.—(1) *Goldscheider's 'Secondary Pressure.'* — Take the shaft of a pin loosely between the finger and thumb of the right hand, and bring the point down sharply but lightly upon the skin of the back of the left hand, or upon the left wrist. You get — if not at once, after two or three trials — two sensations: a primary pressure sensation, followed at an interval of something under a second by a sharper, more thrilling sensation. — Goldscheider, Archiv f. [Anat. u.] Physiol., 1891, 168 f.; Külpe, Outlines, 91; Sanford, Course, exp. 11.

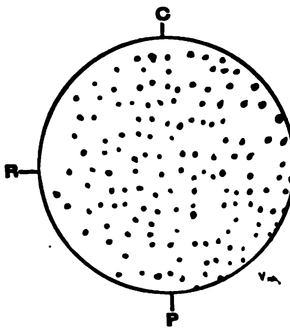


FIG. 10. — Pain spots.

To determine the precise nature of this 'secondary' sensation, proceed as follows. (*a*) Stimulate a pressure spot that has no pain spot in its neighbourhood. The secondary sensation is absent. (*b*) Stimulate a pain spot in the near neighbourhood of a pressure spot. Goldscheider's two sensations will be observed. (*c*) Stimulate a pain spot that is well isolated from pressure spots. The first sensation is absent; the second sensation will be clearly observed. — It follows, then, that the primary sensation is pressure proper, and the secondary sensation a sensation of pain. See von Frey, Abh., etc., 243.

(2) *Electrical Stimulation of the Pressure and Pain Spots.* — The organs of pressure and of pain respond very differently to stimulation by the interrupted current. The pain spot replies by a steady, continuous sensation; the pressure spot by a whirring or hammering, as if a tuning-fork were vibrating upon the stimulated point.

We may use for the experiment the induction coil and one of the cells required for Exp. XX. The arrangement of the inductorium must, of course, be different: *cf.* the diagram. The large-neck electrode will serve here, too, as indifferent electrode: it may be tied upon the left lower arm. For the active electrode we take a piece of thin, soft-copper wire, 10 cm. in length, bent upon itself at right angles near the end, and fused at the

tip into a tiny bulb. This electrode is made the negative pole of the break-shock. It may be applied to the hairy part of the back of the left hand, though better results will be obtained from the leg, where the hairs are set farther apart. As we gradually bring the secondary towards the primary coil, passing the electrode meanwhile over the cutaneous surface, we get here and there a distinct pain sensation. The pain increases, as the secondary coil comes farther in, until it reaches the intensity of a feeling of injury, as if the skin were being torn with a needle. The intervening skin spaces give no sensation. If the electrode travel over a hair-bulb, there may be a sensation of pain, though as a rule there is none. When the pain has reached a decidedly unpleasant intensity, the pressure spots come into play. Their reaction is quite unmistakable.

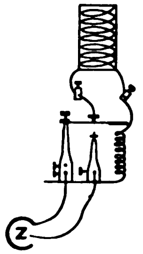


FIG. 11. — Shows the arrangement of the inductorium for (ordinary) repeated shocks. See A. Waller, An Introduction to Human Physiol., 1891, 315; and *cf.* Fig. 16.

A pretty comparative experiment may be performed as follows. Determine the distance between the coils at which you can just sense the pressure-hammering when the active electrode is placed upon the tongue. Now lay the electrode, first, upon the top of the gum, at the point of emergence of one of the incisor teeth, and then upon the upper surface of the tooth itself. In the former case you get the hammering only; in the latter, a steady pain.

Note that upon the palm of the hand, where the epidermis is very thick, the first sensation to appear is not that of pain, but that of pressure.

Von Frey, Ber., etc., Dec. 3, 1894, 290 ff.

(3) The demonstration of Kiesow's painless cheek-area is always interesting to students. See Kiesow, Philos. Studien, ix., 1894, 512; xiv., 1898, 567; von Frey, Ber., etc., Dec. 3, 1894, 293.

LITERATURE. — M. von Frey, Berichte d. math.-phys. Classe d. kgl. sächs. Ges. d. Wiss. zu Leipzig, July 2, 1894, 185; Dec. 3, 1894, 283; Abhandl. d. math.-phys. Classe d. kgl. sächs. Ges. d. Wiss., xxiii., 3, 1896, 239, 251. Von Frey's proof that the pain organs lie more superficially than the organs of pressure and temperature, and his theory of pain stimulation (*cf.* Titchener, Outline, 73), should be fully explained in a lecture.

CHAPTER IV

Gustatory Sensation

§ 26. **Gustatory Sensation.** — F. Kiesow has attempted to arrange the taste qualities in a schema, of the same nature as the colour circle (Philos. Studien, xii., 1896, 273). The taste circle has two diameters, a vertical and a horizontal. Above and below stand salt and sweet; to left and right, bitter and sour. Along the periphery are arranged the mixed qualities salt-sour, sour-sweet, etc. The horizontal diameter represents the bitter-sours; the lower half of the vertical diameter represents the salt-sweets, the upper half the insipid alkaline mixtures. Wundt has adopted this schema (Outlines of Psych., trans. by C. H. Judd, 1897, 53), together with the underlying idea that the sensations of taste form a continuum of two dimensions.

The author doubts whether, in the present state of our knowledge, this idea can be accepted. He doubts, *e.g.*, whether the sweet-sour of lemonade stands to its originals as blue-green stands to blue and green, or as orange to red and yellow; and also whether bitter should lie in the same plane with the other three taste qualities. We must suspend judgment: in the meantime, Kiesow's figure provides us with a working hypothesis.

On taste sensations in general see Wundt, Phys. Psych., i., 1893, 438; M. von Vintschgau, Hermann's Hdbch. d. Physiol., iii., 2, 1880, 145; Külpe, Outlines, 96; Titchener, Outline, 62; Foster, Text-book of Physiol., iv., 1891, 1397.

EXPERIMENT XIV

§ 27. **Distribution of Taste Sensitivity over the Tongue.** *Cautions not noted in the Text.* — It should hardly be necessary to impress upon the student the need of cleanliness in taste-work. The handkerchief should never be used to wipe the tongue, or to dry a brush: it should stay in the pocket. The mouth should

be freed from all food particles; and the student should avoid the taking of any strong-smelling food, or of any considerable quantity of strongly tasting food, shortly before the experiment: in the former case, he becomes disagreeable to *E*, and in the latter the organ may be partly exhausted before work begins. The brushes should either be thrown away after use, or (if they must be used again) carefully washed and disinfected.

It is essential that these experiments be performed in a good light. No special directions need be given about the lens; any large 'magnifying glass,' clamped in the proper supports, will serve the required purpose. The lens used in the Cornell Laboratory is one of 12.5 cm. diameter and 25 cm. focus. If *O* prefers to stimulate his own tongue, a concave (enlarging) glass mirror may replace the lens. The filled brushes are then handed by *E* to *O*, who applies them to the required papilla, under the guidance of the mirror image.

Kiesow recommends brushes of 8 mm. length, and a mean diameter (when wetted) of 1 mm.; Oehrwall, brushes of 2 cm. length and 5 mm. diameter. We have obtained the best results with brushes of 2.5 to 3 cm. length, and 5 mm. diameter at the insertion of the handle. The point must be trimmed with the greatest nicety. When the brush has once been dampened, the merest trace of extra liquid is sufficient to stimulate the papilla.

If need arise, the strength of the solutions may be varied. Sanford recommends (weak) sugar, 5%; (strong) sugar, 40%; tartaric acid, 5%. A few preliminary trials will decide the matter. The solutions should, in summer, be kept at the temperature of the room in which the experiments are made; in winter, slightly warmed.

It is necessary to assure oneself (by preliminary trials) that the distilled water is tasteless. It should not be difficult to procure water (distilled or other) which is reported as without taste; but the tastelessness must not be taken for granted. A particular *O* may sense distilled water as quite noticeably sweet or sour or bitter; and one and the same *O* may report different tastes at different parts of the tongue. The author has never known a case in which distilled water has given a salt taste.

e both sour and sweet 'contrast' with salt (see Exp. XVI.), ce of salt may neutralise the water for an *O* who gets the or sweet sensation. Bitter, unfortunately, does not con- with any other taste, and hence cannot be eliminated by pensation. It remains, then, in certain rare cases, as a con- source of disturbance in the experimental series.

XPERIMENT. — Extreme care must be taken that the drop in the brush does not spread to other papillæ than that r stimulation. The spreading may be due to excess of d, or to the presence of saliva upon the surface of the organ. he tongue might, of course, be dried by pressing with cotton or a fine cloth immediately upon exposure; but the result ually a too speedy evaporation, and a consequent reduction he sensitivity of the papillæ. With care, the squeeze st the roof of the mouth answers well. — Individuals differ ly in their power to hold the tongue steady. But practice gives the required control.

tice that bitters are more lasting than other tastes. Hence, ever a bitter comes in the series, a longer pause than the mary 2 to 3 min. must be made.

nagination' and 'suggestion' may play a large part in this riment: hence the necessity of keeping *O* in ignorance of nature of the stimulus and the results obtained with previ- applications.

UESTIONS. — (1) Yes; although the evidence is not easy of titative interpretation. If all doubtful cases in the Table ?-judgments) are omitted, it will probably be found that cer- papillæ are sensitive only to sweet; possibly, that some sensitive only to salt or acid: it is not likely, at any rate in area of the tongue, that a papilla will be found which is usively sensitive to bitter. On the other hand, several will ably be insensitive to bitter; some, perhaps, to salt and ; and the whole region may possibly prove to be insensitive weet. Other combinations of sensitiveness and insensitive- s will probably be found, but can hardly be predicted. It is ural that differences should obtain, seeing that the papilla is unch or cluster of taste-cells, and that in these, if in any part the peripheral organ, the specific taste energies would reside.

Question (1) may be extended, as follows. Can you infer anything, from the experimental results, as to the function and distribution of the ultimate end-organs of taste (the taste-cells in the beakers)?

(2) The results vary considerably (at least in the early stages of practice) from individual to individual. It is, however, probable that salt and acid will be readily confused. This is natural if, as has recently been argued, sweet and bitter are the primitive taste sensations. *Cf.* also the nature of their concomitant sensations, mentioned under (3).

(3) In many experiments *O* will report a pressure, temperature (warm or cold) or pain (stabbing, biting, burning) sensation. These concomitant sensations will be characterised somewhat as follows. (1) Sour is at first astringent; then, as it becomes stronger, burning; finally, purely painful. (2) Salt is attended by a weak burning, not rising to positive pain. (3) Sweet brings with it the perception of smoothness and softness. At high intensities of stimulus, it pricks or gives a sharp burn. (4) Bitter suggests something fatty. At high intensities, it may burn.

(4) Bitter is set up noticeably later than sweet or acid. Since *O*'s attention in this experiment is directed mainly upon the quality of the aroused sensation, the time-difference may escape him. It occasionally happens, however, that a single stimulus calls forth a mixture of sensations. In such cases, the simultaneity or succession of the components can be noted. Thus acid may evoke a sour-salty taste (simultaneous); a bitter may evoke a slight sweet followed by a bitter. The reasons for the mixture cannot be given with any certainty. But associative processes, central or peripheral (*cf.* above) or both, are always to be suspected.

(5) Associative processes, as just remarked. Also peripheral fatigue; especially in the case of the bitter and sweet (strong) solutions.

The second of these we seek to rule out by allowing an adequate time-interval to elapse between experiment and experiment. The former we combat by the introduction of the experiments with distilled water — 'blank' experiments or

'puzzle' experiments, as they are called. The object of these experiments is to hold the attention of *O* rigorously upon the stimuli; a habit of judgment, formed under the influence of associations, will receive a rude shock when there is absolutely no sense-material offered around which the associations may group, — and so *O* will be 'waked up' to a more objective attitude.

(6) i. *The Taste-effects of Mechanical and Electrical Stimulation of the Papillæ.* — *Mechanical* stimulation may easily be tried, by help of the pressure-point of Exp. XII. No sensations of taste will be obtained. Notice, however, that a pressure upon the base of the tongue gives a distinctly bitter sensation. This may be a mechanically aroused, peripheral sensation; or may be an associative process, attached to the choking and nausea that follow from the application of stimulus. The question whether *electrical* stimulation of a papilla sets up a taste sensation, over and above the sensations indirectly aroused by decomposition of the saliva, is still undecided, and is very difficult of decision. Unless some weeks can be given to the repetition of the principal experiments (see References) the problem is better left untouched.

ii. *The Taste-reactions of Other Forms of Papillæ.* — (*a*) Fill a brush with one of the (weak solution) liquids, and paint lightly over an area of the circumvallate papillæ. All four tastes will be obtained. The fungiform papillæ are, in fact, only less highly developed forms of these circumvallate papillæ. (*b*) Set a drop of liquid (strong solution) upon the filiform papillæ, taking care that no fungiform papilla is affected. No taste results.

iii. *Proof of the Independence of the Four Taste-qualities by Elimination of Each singly.* — (*a*) Find a papilla which is extremely sensitive to bitter. Paint it a few times over with a 10 to 20% solution of cocaine hydrochlorate. Notice that, while the bitter taste is entirely abolished, the other tastes of which the papilla is capable remain. (*b*) If the papilla is capable of sweet and salt, or sweet and sour sensations, over and above the bitter, continue the painting. Notice that the salt (or sour) sensation persists, after sweet has undergone the same fate as bitter. (*c*) Find a papilla which is extremely sensitive to sweet.

Paint it over with a saturated alcoholic solution of gymnemic acid. Notice that the other qualities of which the papilla is capable remain after the abolition of the sweet sensation.

RESULTS.—The following results, showing the sensations recorded in single series (*not* averages) from four papillæ, may be taken as typical for an unpractised *O*:

(1) Weak solutions:

Papilla.	Sugar.	Salt.	Acid.	Quinine.	Water.
1	+	sour	—	+	—
2	?	—	—	?	very faint bitter
3	?	sour	salt	—(cold)	—
4	+	+ (weak)	slight salt	—	—

(2) Strong solutions:

Papilla.	Sugar.	Salt.	Acid.	Quinine.	Water.
1	+	+	salt	—(sting)	—
2	—	—(sting)	—	++	—
3	+	+	?	++	—(cold)
4	+	+	salt	++	—

(3) Weak solutions, with spread of stimulus owing to too large brush:

Papilla.	Sugar.	Salt.	Acid.	Quinine.
2	+	++	+	weak sour
3	+	+	salty bitter	?
4	++	+	++	+
5	+	++	+	—

LITERATURE.—H. Oehrwall, Untersuchungen über den Geschmackssinn. In the Skand. Archiv für Physiologie, 1890, ii., 1.

F. Kiesow, Schmeckversuche an einzelnen Papillen. In Wundt's Philosophische Studien, 1898, xiv., 591.

F. Hofmann and R. Bunzel, Untersuchungen über den elektrischen Geschmack. In Pflüger's Archiv für die gesammte Physiologie, 1897, lxvi., 215.

R. von Zeynek, Ueber den elektrischen Geschmack. In the Centralblatt für Physiologie, 10 Decr., 1898, xii., 617.

W. Sternberg, Zeits. f. Psych., xx., 1899, 385.

EXPERIMENT XV

§ 28. **The Number of Taste Qualities.** *Cautions not noted in the Text.* — All odorous solutions should be in narrow-mouthed phials, and should be kept carefully corked when not in use, to avoid diffusion of their odour, and the consequent possible recognition by *O* in inspiration. They should be of the temperature of the room, in summer; slightly warmer, in winter. The phials should, further, be covered with paper, so that the solutions cannot be distinguished by differences of colour. The following 'tastes' are easily procurable:

Syrup of Orange. 15 % solution.	Clam Bouillon.
Lime Juice. 10 %.	Milk.
Tar Water (made from 5 % Wine of Tar).	Tea.
Syrup of Sarsaparilla. 15 %.	Coffee.
Solution of Powdered Alum. 2 %.	Vinegar.
Essence of Wintergreen. 5 %.	Mushroom Catsup.
Syrup of Lemon. 15 %.	Pineapple Syrup. 15 %.
Syrup of Cherry. 15 %.	Essence of Peppermint. 2 %.
Essence of Sassafras. 5 %.	Gum Arabic.
95 % Alcohol. 33 %.	Chocolate.
Peach 'fruit flavour.' 5 %.	Lime Water.
Essence of Bitter Almonds. 2 %.	Lithia Water.
Essence of Anise. 5 %.	Tomato Catsup.
Epsom Salts. 2 % of crystals.	Maple syrup.
Beef Bouillon.	Toast and water.

QUESTIONS. — (1) No. No stimulus is recognised. Nothing is sensed but the four tastes, with pressure (tingling, pricking), temperature (cold, burn) and possibly — if the solutions be too strong — pain accompaniments.

(2) Partly to avoid peripheral fatigue; partly to rule out all chances of associative influence upon judgment; partly to counteract the variable errors of habituation and expectation.

(3) In order that introspection may not be influenced by an odour sensed in the act of inspiration. *O*'s nostrils must be very tightly plugged, for the same reason.

(4) Yes. The ***name*** of the stimulus would bring a host of associations with it, and a pure experiment would then be impossible.

RESULTS.—The following results may be taken as typical:

Solution.	Taste reactions of a single papilla, on different days.
Tar	(1) Sharp, salty taste; (2) Salty.
Anise	(1) Very sharp; (2) Sour, with sharp burn.
Alcohol	(1) Salt and bitter; (2) Salt.
Lemon	(1) Burns: sour or salt; (2) Sharp burn.
Wintergreen	(1) Sweet; (2) Sharp taste.
Alum	(1) Very slightly salt; (2) Nothing.
Bitter Almond	(1) Bitter: burns; (2) Bitter: burns.
Cherry	(1) Sweet; (2) Sweet.
Peach	(1) ? Cold; (2) Cold.
Epsom salts	(1) Salty; (2) Burns.
Orange	(1) Sweet; (2) Nothing.
Sarsaparilla	(1) Sour, and slightly bitter; (2) Bitter.
Pineapple	(1) Sweet; (2) Sweet.
Etc., etc.	

EXPERIMENT XVI

§ 29. **Taste Contrasts.** *Cautions not noted in the Text.*—'Strong' and 'weak,' as applied to taste solutions, are doubly relative terms. (1) Individuals differ. Thus, if the 30 % sugar solution give no sensation beyond a 'sharp burn,' it will plainly be necessary to reduce the strength of the standard sweet. (2) Practice makes a great difference. Thus, in the early stages of taste-work, a salt solution of 2 % sat. sol. may be subliminal.—The Instructor must not expect, then, that the ***numerical*** results of this experiment will show any great uniformity, as between different students.

O knows the quality of the standard and of the weaker solutions, but should not know whether one of the weaker solutions or merely water is to be expected in a given case.

Exact simultaneity in the application of the two stimuli is not necessary. If there is any time difference, the standard solution must, of course, be applied first.

RESULTS. — The following results may be taken as typical:

Standard: 50% sat. salt sol.

Dist. water is sensed as:	nothing; bitter with suggestion of sweet; mere suggestion of sweet; very slightly sweet.
Subl. sugar sol. " "	suggestion of sweet; faint, slight, weak sweet; good sweet.
Weak sugar sol. " "	sweet; good sweet; very sweet.

Standard: 30% sat. sugar sol.

Dist. water is sensed as:	nothing; faint bitter; suggestion of salt in moment of application, then nothing; slight bitter-salty; suggestion of salt.
Subl. salt sol. " "	weak, faint salt; good salt; very salt.
Weak salt sol. " "	decided salt; very salt.

QUESTIONS. — (1) We find (*a*) that distilled water, previously tasteless, becomes faintly salt or sweet, as contrast requires. (*b*) A subliminal solution gives a clear, and at times a strong, sensation. (*c*) The weak solutions give quite strong sensations.

Now it is doubtless true that *O*, in spite of our initial caution, is *expecting* a contrast-sensation. But *O* does not know when the water stimulus is coming; and has no reason, *a priori*, to suppose that water will be sensed by taste at all. Indeed, the use of water in the blank experiments of Exps. XIV. and XV. would rather suggest that water will not be tasted. As we find that water sometimes gives 'nothing' and sometimes something, — expectation remaining the same, — we may be pretty confident that a real contrast is present when a taste is set up. Again: a record like the "suggestion of salt in moment of application, then nothing," given above, is evidence that *O* is well on his guard, and able to distinguish the 'suggested' from the peripheral sensation. The expectation-taste, so to call it, is negatived by introspection. Moreover, there are enough dis-

turbing factors in the experiments to upset any hard-and-fast expectation. Thus, as we see in the above results, both sugar and salt may make distilled water taste bitter. The phenomenon is of not infrequent occurrence, and is very difficult to explain, since bitter (as we shall see below) does not contrast with any other taste. (*a*) Possibly, in some instances, the 'bitter' may be an associative process due to verbal suggestion: language opposes 'bitter' to 'sweet,' quite definitely. (*b*) In other cases, it may be that the effort of holding out the tongue involves something like a choking or incipient vomiting reflex, so that the associative bitter of the base of the tongue comes into play. (*c*) It is noteworthy, too, that the 'bitter' of distilled water is often designated a 'smooth bitter.' It may be, then, that the 'smoothness' of the water, as distinguished from the weak burning characteristic of salt of all intensities, and the sharp burn characteristic of sugar solutions of high intensity, suggests the 'fatty' concomitant of bitter (see p. 102), and, by that means, bitter itself. — Further (though this fact does not appear in our Table), the standard sugar solution will, at times, induce not salt but sweet, its own quality. In view of these irregularities, we may safely assume that an obstinate 'expectation' of contrast would be broken up in the course of the experimental series, and that the contrast-effects, when obtained, are what they profess to be. — We shall return to the point below.

(2) The sweet induced by the salt is, at least for most *O*'s, stronger and clearer than the salt induced by the sugar.

(3) The following experiments suggest themselves.

(*a*) We have used a strong solution as the inducing taste: as if on the assumption that, in taste as in sight, the more saturated quality will provoke the stronger contrast-effect. It would be well, now, to try the inducing power of weak, just supraliminal, solutions. If these are able to colour distilled water with the contrast-taste, and to raise a subliminal taste above the limen, our belief in the contrast-phenomenon at large will be increased: for one would hardly 'expect' so definite a result from so weak a stimulus.

It may be said that all the contrasts obtainable from strong are also obtainable from weak solutions. Indeed, for some *O*'s,

the weak solutions induce better than the strong; for the effect of the strong stimulus is to draw the attention to itself, and away from the quality of the weaker (contrast) sensation.

(*b*) It would be well to test other taste qualities, with a view to the ascertainment of their contrast relations. If this is done, we find:

i. that salt and sour contrast: the sour induced by salt being clearer and stronger than the salt induced by sour;

ii. that sweet and sour contrast: the sweet induced by sour being clearer and stronger than the sour induced by sweet;

iii. that bitter shows no contrast at all: subliminal bitter, if applied simultaneously with sweet, sour or salt, is always sensed (when it is sensed at all) as sweet; and supraliminal bitter is from the very first strong and insistent.

If we add to this summary the result of the foregoing experiment:

iv. that salt and sweet contrast: the sweet induced by salt being clearer and stronger than the salt induced by sweet; we come upon the general rule of taste-contrasts, that the order of qualities, as regards ease of induction, is sweet, sour, salt, bitter.

(*c*) It would be worth while to test the simultaneous by the successive method. In this, *E* drops the standard solution upon the tip (not the side) of *O*'s tongue. The liquid is left in place for 3 sec. *O* then washes out his mouth, vigorously and thoroughly, with distilled water. When all trace of the former sensation has disappeared, *E* applies the second (weaker) stimulus to the same part. *O*'s judgments of this second stimulus are recorded.

The experiment, in this form, should be tried with various intensities of inducing stimulus, and with all the taste qualities. It will be found that contrast is here less readily set up (*i.e.*, that it takes a stronger inducing stimulus to evoke it) than in the simultaneous method. The pauses between experiments must be regulated by the intensities of stimulus employed; but the mouth should be rinsed for at least 30 sec., even when the inducing taste is barely supraliminal.

The results of the previous experiments will be confirmed.

(*d*) It should be possible, by aid of a long series of intensively graded solutions of the contrast-taste, roughly to measure the effect of the inducing solution. Thus, if a 5 % salt sol. has been judged (by contrast) as 'good salt,' the mouth could be violently rinsed, and then this same intensity 'good salt' matched (without contrast) from the series of graded salt solutions. — The experiment would, however, be tedious, and its results not very accurate. Still, it might be assigned as a problem to an interested student.

Related Experiments. — (1) We spoke, in Exp. XIV., of 'neutralising' the taste of distilled water by adding salt to it, — as if the sweet or sour taste could actually be cancelled by the addition of a stimulus of contrasting quality. The experiment may now be tried for its own sake. A 20 % sugar solution, *e.g.*, may be taken, and changed from experiment to experiment by the intermixture of a small quantity of saturated salt solution. The student may be left to regulate the time-interval between test and test, and to determine the amount of salt to be added to a given quantity of sweet. Does the solution reach a stage of complete gustatory indifference? Does it pass at a jump from sweet to salt? Or is neither of these alternatives realised, but a new taste altogether set up with intensive equality of the two primary tastes?

These questions are differently answered by different observers. Taste is subject to enormous individual variation, and this particular experiment shows the variation in its extremest form. Some observers get a neutralisation even with *bitter* and sweet, although bitter, for the same observers, shows no trace of contrast-effect! Others get nothing more than the (more or less abrupt) change of primary taste qualities. One result, however, comes out pretty constantly: that a compensating mixture of sweet and salt gives rise to an 'insipid,' 'flat,' alkaline taste, entirely distinct from that of the two components.

(2) This result suggests a further experiment, — the synthetising of the two mixed tastes, alkaline and metallic, which have played so large a part in the discussions concerning the number of discriminable taste qualities. The alkaline and metallic tastes

contain (*a*) the taste of the mixture of two or more of the true taste qualities, and (*b*) a certain complex of pressure and other concomitant sensations. Acting on the hint that sweet and salt, in proper proportion, give an alkaline flavour, the student may set to work accurately to synthetise or reconstruct this and the metallic taste. His first step is a careful introspective analysis of certain solutions that give these tastes. Then he begins his reconstruction as systematically as the outcome of introspection and the facts of the preceding experiments allow.

LITERATURE. — M. von Vintschgau, Hermann's Handbuch d. Physiol., III, 2, 219 f. A. Goldscheider and H. Schmidt, Goldscheider's Ges. Abh., i., 1898, 382 (synthetises the alkaline taste from *bitter*, salt and 'sensible Erregung'); Wundt, Outlines of Psych., 53 (suggests that alkaline = salt and sweet, metallic = salt and sour).

Cf., also, G. T. W. Patrick, Iowa Studies in Psych., ii., 1899, 85; R. W. Tallman and H. Gale, Gale's Psych. Studies, i., 1900, 118.

CHAPTER V

OLFACTORY SENSATION

§ 30. **Olfactory Sensation.** — EXERCISE (1). — This test need not be given if *O* is sufficiently impressed by Exp. XV. As a rule, however, there is still some scepticism remaining, after the papilla work, which can be dispelled only by work in the gross. Identification is quite impossible in terms of taste alone.

(2) Both substances 'smell sweet.' So strong is the association, that the realisation of the actual taste comes with a shock of surprise even to a practised observer.

(3) Zwaardemaker distinguishes nine smell classes :

(1) Ethereal scents. All fruit odours.
(2) Aromatic scents. Camphor and spicy smells ; anise, lavender, etc.
(3) Fragrant scents. Flower odours ; vanilla ; gum benzoin, etc.
(4) Ambrosiac scents. Amber ; musk.
(5) Alliaceous scents. Garlic, asafœtida ; bromine, chlorine, etc.
(6) Empyreumatic scents. Toast, tobacco smoke ; naphtha, etc.
(7) Hircine scents. Cheese, sweat, etc.
(8) Virulent scents. Opium, cimicine, etc.
(9) Nauseous scents. Decaying animal matter, fæces, etc.

Not all of these can, perhaps, be represented in the laboratory (see however, p. 127 below). The following list will, however, be found fairly practicable. The letters 'e. o.' following the name of the substance denote 'essential oil'; *L.* means 'De Laire specialty' (formula unknown); 't.' means 'alcoholic tincture'; and 't. a.,' 'trade article.' Substances grouped by Zwaardemaker are italicised. The others have been classified by Dr. E. A. Gamble. Disagreements with Zwaardemaker are indicated in brackets.

SUBSTANCE.	Place in Zwaardemaker's list (pp. 233–235)	SUBSTANCE.	Place in Zwaardemaker's list (pp. 233–235)
Almond, e. o.	(2) *e*.	Lactic acid	(7) *a*.
Amber, e. o.	(4) *a*.	*Laudanum*, t.	(8) *a*.
Ammonium sulphide	(5) *a*.	*Lavender*, e. o.	(2) *c*, γ;
Anise, e. o.	(2) *c*, α.		[(2) *a*?].
Asafœtida	(5) *a*.	*Lemon*, e. o.	(2) *d*, β.
Aubepine, *L.*	(3) *a*.	Lilac, t. a.	(3) *b*.
Benzine	(6) *b*.	*Methyl alcohol*	(6) *b*.
Benzoin	(3) *c*.	*Musk*, t.	(4) *b*.
Bergamot, e. o.	(2) *d*, β.	Mustard, e. o.	(2) *b*?
Birch, e. o.	(2) *c*, β.	*Nutmeg*, e. o.	(2) *b*, β.
Butyric ether	(2) *c*;	*Orange*, e. o.	(2) *d*, β.
	[(1) *a*?].	Orris	(3) *b*, β.
Calamus, e. o.	(2) *c*, β?	*Oxalic ether*	(1) *c*.
Caraway, e. o.	(2) *c*, α.	Parsley, e. o.	(4) *a*.
Carbon disulphide	(5) *a*;	*Patchouli*, e. o.	(2) *a*;
	[(9) *a*?].		[(4) *a*?].
Caryophylline, *L.*	(2) *b*, α.	Pennyroyal, e. o.	(2) *c*, β.
Cassia, e. o.	(2) *b*, β.	*Pepper*, e. o.	(2) *b*, α.
Cheese, stale	(7) *a*.	*Peppermint*, e. o.	(2) *c*, β.
Cinnamon, e. o.	(2) *b*, β.	*Pine needles*, e. o.	(2) *a*.
Citronella, e. o.	(2) *d*, β.	*Pyridine*	(6) *a*.
Clematite, *L.*	(3) *b*.	Quarantaine, *L.*	(3) *b*?
Cloves, e. o.	(2) *b*, α.	Rhubarb, t.	(8) *a*.
Clymene, *L.*	(3) *a*.	*Rose*, e. o.	(2) *d*, α.
Cocoa butter	(2) *b*?	*Rosemary*, e. o.	(2) *a*.
Coffee	(6) *a*.	*Rosewood*, e. o.	(2) *d*, α.
Cologne, t. a.	(3) *c*?	Rue, e. o.	(2) *a*?
Coumarine, *L.*	(3) *c*.	Sage, e. o.	(4) *a*?
Crab-apple blossom, t. a.	(3) *a*, β.	*Sandalwood*, e. o.	(2) *d*, β;
Creosote	(6) *b*.		[(4) *a*?].
Cubebs, e. o.	(2) *b*, α.	Sassafras, e. o.	(2) *c*, β?
Cummin, e. o.	(2) *c*, α.	Spearmint, e. o.	(2) *c*, β.
Cuir de Russie, *L.*	(4) *a*?	*Syringa*, *L.*	(3) *a*, β.
Ether, sulphuric, t. a.	(1) *c*.	*Tar*	(6) *b*.
Eucalyptus, e. o.	(2) *a*.	*Tea*	(3) *b*, β.
Foul alcohol (poured off		*Thyme*, e. o.	(2) *c*, γ;
specimens)	(9) *a*.		[(2) *a*?].
Garlic, e. o.	(5) *a*.	*Tobacco*	(6) *a*.
Gasoline	(6) *b*.	Valerian, t.	(7) *a*.
Geranium, e. o.	(2) *d*, α.	*Vanilla*, t.	(3) *c*.
Heliotropine, *L.*	(3) *c*.	*Violet*, t. a.	(3) *b*, β.
Hemerocalle, *L.*	(3) *b*.	Wintergreen, e. o.	(2) *c*, β.
Jacinthe, *L.*	(3) *b*, α.	Wych-hazel, t. a.	(2) *a*?
Juniper, e. o.	(2) *c*, α?	Yara yara, *L.*	(3) *a*?

It should be said that the proposed departures from Zwaardemaker's classification are based upon actual confusions found in experimental work. Thus, amber and patchouli are confused, as are thyme, lavender, pine needles, eucalyptus and rosemary, in experiments upon smell memory and recognition. The *L.* substances are usually too strong for work, — so strong as to be all much alike, — and must therefore be diluted.

LITERATURE. — On olfactory sensation in general, see Wundt, Phys. Psych., i., 1893, 441; Külpe, Outlines, 100; M. von Vintschgau, Hermann's Hdbch. d. Physiol., iii., 2, 1880, 225; H. Zwaardemaker, Die Physiol. d. Geruchs, 1895; Titchener, Outline, 61; Foster, Textbook of Physiol., iv., 1891, 1388.

EXPERIMENT XVII

§ 31. **The Field of Smell.** — It may be said at once that the statement in the text "It [the field of smell] cannot be larger than the breathing field," while it is obviously true, may not be borne out by the results of this experiment. *O* is required to smell voluntarily, *i.e.*, to sniff; and sniffing expands the alæ of the nostrils. It may quite well be the case, then, that the breadth of the field of smell, as mapped by *E*, is slightly greater than the breadth of the field of breathing.

An objection to the experiment is that the horizontal arrangement of tin and paper offers an obstacle to free inspiration, while it unduly favours the taking-in of air from the sides. To this Zwaardemaker replies (Physiol. d. Geruchs, 70) — and the author is able to confirm the statement — that the fields of smell are not appreciably larger when a sheet of wide-meshed gauze replaces the tin or paper. Moreover, inspiration of the kind required is not by any means unnatural: *cf.* our normal smelling of a flower, a glass of wine, or a plate of food; or a dog's following of a trail.

PRELIMINARIES. — It is necessary that the grip of the teeth be precisely the same throughout the experiment. The paint line may be marked with little cross-lines, indicating the position of *O*'s front teeth; or the tin may be indented, to take the teeth; or, finally, the wax may be left in place from one experiment to another. Different *O*'s prefer different methods. For

paint, use 'drop black,' a dead-finish paint which is often useful in the laboratory.

EXPERIMENT (1). — A perfectly normal nose is rather the exception than the rule. *E* must not, therefore, be surprised at irregularities in the outline of the spots, at differences of size and shape as between the right and left areas, at the occasional absence of the oblique cross-line, etc. Zwaardemaker seems not to have observed this last anomaly (p. 73): it has, however, occurred more than once in the author's experience. Moreover, the secondary division often runs down and out, instead of down and in. Dr. Gamble writes to the author: "A very large number of my records (and I have now examined the breathing spots of more than 100 people), though not, I think, the majority, show the secondary division at right angles to the normal."

EXPERIMENT (2). — The need of strict control of the smell stimulus cannot be too strongly impressed upon the student. The syringe must be perfectly oil-tight, and perfectly free from odour when the point of the needle is closed. After, say, every eight tests, the room in which the experiment is performed must be thoroughly aired. On the other hand, doors and windows must be tightly closed during the tests, since even a slight draught will very considerably derange the field of smell. If the field of smell extends beyond the field of breathing on one side, or runs into the intermediate area in one direction, there is probably a draught at work. The place of the apparatus in the room should then be changed.

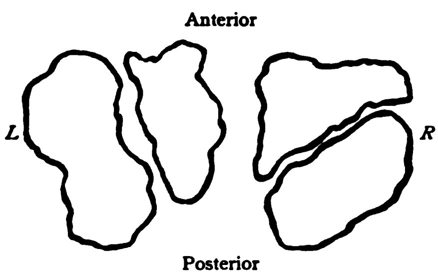


FIG. 12. — Normal breathing spots (Zwaardemaker). The diagram is printed upside-down in the Physiol. d. Geruchs, p. 72; cf. the text of pp. 72 f.

It may be necessary to make a series of preliminary experiments, in order roughly to determine the liminal stimulus, *i.e.*, the movement of the syringe-piston which just gives rise to a sensation of determinate quality, in the middle region of the

smell field, during the 2 sec. limit. This movement has varied, in the author's experiments, between 2 and 7 mm. The rate of pushing the piston must be kept as constant as possible, as well as the distance of push and the time of exposure of the needle point.

Note that the ready-signal is to be given *after* the needle has pierced the paper. Otherwise, the noise of the prick may distract *O*'s attention from the scent. To avoid fatigue, it is well to take only eight tests in a series, and to distribute these, four to each nostril, in irregular order. The exploration of the breathing field must also be entirely irregular.

RESULTS. — The following diagrams, imperfect as they are, represent the average result attainable in the limited time that

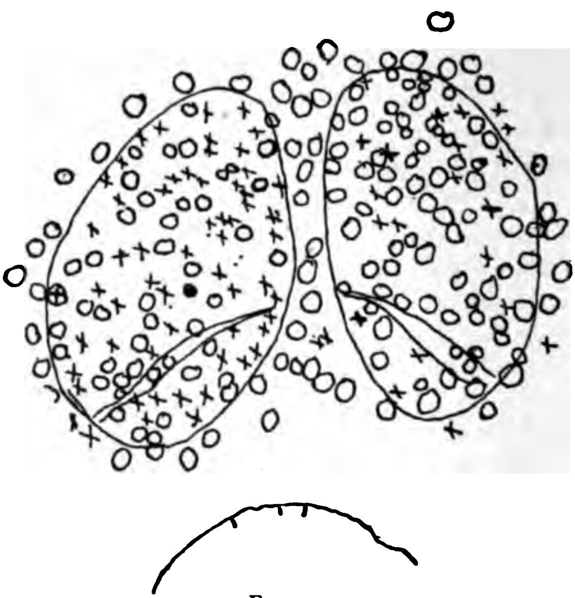


FIG. 13.

can be given to the experiment. In Fig. 13, the secondary divisions are at right angles to the normal; in Fig. 14 no secondary division could be found. The positive errors (sensations where there should have been no sensation) were in nearly every case explicable (after the event!) by draughts: it must be remem-

bered that any considerable movement, say, of head or arm, on the part of *E* or of *O*, sets up air-currents in the experimenting room. The negative errors can be explained only conjecturally.

Questions.—(1) Not, at any rate, for all *O*'s. There are, in very many cases, patches where 'something' is smelled, which is not distinctly oil of cloves. This change of quality must

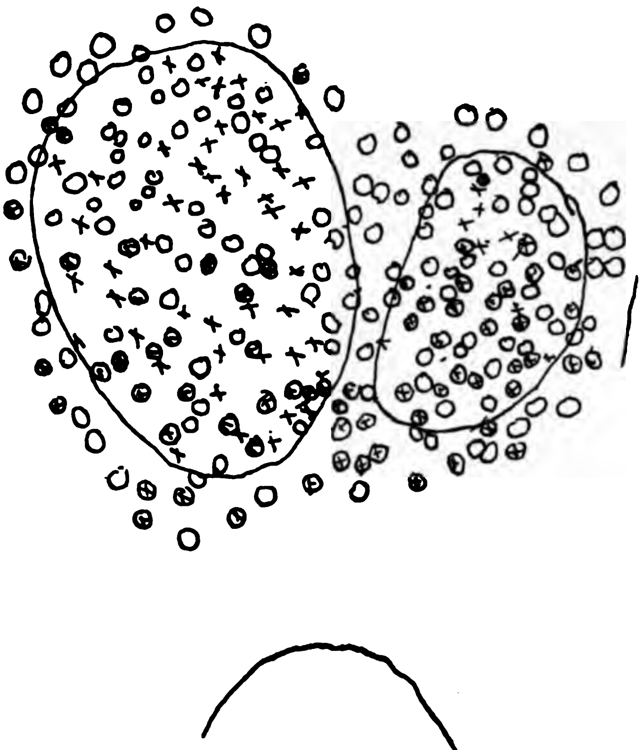


Fig. 14.

apparently be attributed to a change in local sensitivity of the olfactory mucous membrane.

(2) The field of smell is the smaller of the two. In a perfectly conducted experiment, it would correspond to the antero-median portions of the breathing field. The possibly greater breadth of the field of smell has already been remarked on and explained (p. 114).

Beyond the breathing field there is no smell. Nor do smell sensations come from the intermediate strip that corresponds to the septum, or from the strips that divide the anteromedian from the posterolateral portions of the breathing field.

(3) To avoid the diffusion error. If, *e.g.*, the needle point be held for some little time close to the lips (a point from which no sensation of smell can properly be set up), the oil of cloves volatilises, and the vapour in ascending strikes the breathing cone at an angle. The olfactory stimulus is thus sucked into the inspiration-current, and will presently arouse an olfactory sensation.

Zwaardemaker (p. 69) recommends a stimulus-time of 1 sec. This answers with practised *O*'s; for unpractised, it is too short.

(4) These have been mentioned above. They are: diffusion of the stimulus in the room, whether by carelessness on the part of *E*, by draughts, by an overlong working without change of air, or by overlong exposure in a single experiment; unequal stimulation, due to variation in the rate or distance of push of the piston; fatigue on the part of *O*; too vigorous sniffing; suggestion or distraction by the sound of the needle-prick in the paper.

(5) See Zwaardemaker, Physiol. d. Geruchs, chs. iii., iv., esp. 72 f.

(6) In ordinary life we move the head in all directions, and are constantly in draughts, or are creating draughts by our movements. Moreover, the cross-section of the breathing cones in this experiment is taken very near their vertices.

Related Experiment. — The statement that the field of smell "may be coincident with . . . or may be smaller than the field of breathing" presupposes *O*'s ignorance of a simple but striking experiment (Fick) in the sphere of olfaction. It is as follows. Introduce an olfactory stimulus — *e.g.*, the pointed end of a paper funnel held over some scented object — into the posterior half of the nostril, and you smell nothing at all; shift the stimulus to the anterior half of the nostril, and you get an intensive smell sensation. It follows from this experiment that the field of smell must be smaller than the inspiration field.

Literature. — A. Fick, Anatomie u. Physiologie d. Sinnesorgane, 1864, 99; H. Zwaardemaker, Die Physiol. d. Geruchs, 1895, 69 ff.

EXPERIMENT XVIII

§ 32. **The Olfactory Qualities: Method of Exhaustion.** — All of the foregoing experiments ought, in the present state of our knowledge, to be carried out in quantitative terms; that is to say, they should be performed with the Zwaardemaker olfactometer, in one form or other, with standardised stimuli and with known intensities of the stimuli. But olfactometric technique — again, in the present state of our knowledge — is so circumstantial and time-taking that insistence on this point would imply neglect of other equally important experiments in other sense departments. It has therefore seemed best, especially as the olfactometer must be employed in the following Experiment, to give the tests in rough form. The following hints will be enough to guide the Instructor, in case any student shows a special aptitude or desire for olfactometric work.

EXPERIMENTS. — (*a*) Take cylinders of beeswax and tolu balsam, which are decidedly exhausting, and of grey india rubber and tallow, which are not particularly so, and work out the experiment described by Zwaardemaker on p. 204 of the Physiologie des Geruchs (paragraph beginning "Bedienen wir uns . . ."), platting curves like those on p. 205. Where the adhesion error enters, a clean inhaling tube should be used for each determination of the limen. Two clinical olfactometers and a supply of tubes must therefore be on hand.

(*b*) Secure complete exhaustion with different intensities of the same quality, using the cylinder on the instrument during the exhaustion process. Plat curves, with the durations of the odour for ordinates, and the intensities of stimulus for abscissæ.

(*c*) The recuperation experiment could be made on the olfactometer with one or two qualities, and one or two intensities of the same quality. Curves should be platted, with the durations of the smells for ordinates, and the numbers of the exhaustions for abscissæ.

If the whole cylinder is used to exhaust, it may be taken off the olfactometer. When only a part of the cylinder is used, — reduced intensity of the quality, — the cylinder should be slipped

over a clean inhaling tube during each interval of rest, to avoid the adhesion error.

(*d*) The main experiment, that upon the determination of the elementary qualities by the exhaustion method, should be performed systematically with the olfactometer, — Zwaardemaker's localisation theory (Physiol. d. Ger., p. 271) being taken as a working hypothesis.

But — the caution may be repeated — work of this kind requires more time than can usually be allowed to Smell in a drill course. We have here nothing that corresponds in ease and accuracy of manipulation to the rotating discs of Optics.

The main objective source of error in the experiments of the text is the propagation of the scents by diffusion. The phials must always be carefully stoppered, except when in use. The scents must be stored in a room or closet away from the experimenting room, and this room or closet must be ventilated by a through-draught. Phials should be brought into the experimenting room as wanted, not all together. The experimenting room itself should be well ventilated. All smell work should, by rights, be done in a room with walls, etc., of glazed tile; at any rate, the walls and ceiling should be covered with glazed paper, and the floor with varnished linoleum, the proper smell of which has worn off. If these conditions cannot be realised, recourse must be had to frequent and thorough ventilating.

If the students are entirely unfamiliar with the properties of smell stimuli, a few preliminary demonstrations may be given without waste of time.

(*a*) Leave a phial of oil of cloves open in a closed room. After two minutes, open the door of the room, and let the student, standing at the door, take two breaths of the diffused scent. Now close the room for another three minutes. Repeat the test. Note the increased intensity of the odour.

(*b*) Compare the power of three drops of oil of cloves to scent a room, (i) when they are at the bottom of a phial, and (ii) when they are smeared upon the surface of a plate.

(*c*) Note the difference in the intensity of the smell of beeswax, or of the pasteboard or leather covers of books, in damp

and in dry weather, or in the moist and dry atmosphere of a room.

(*d*) Note the difference in the intensity of the smell of gum benzoin in a warm and a cold room.

(*e*) The fact of exhaustion can be brought out very prettily as follows. Procure two flowers, — two roses, or carnations, or sprigs of heliotrope, — as nearly as possible of the same size. Let *O* assure himself, by a single sniff, that both give out a strong perfume. Now let him take one of the two (by preference the smaller, if there is any difference of size), and smell hard at it for a few inhalations. Then let him smell at the larger. The latter will give forth very little, if any scent. The test is more striking in the performance than in the reading.

EXPERIMENT (1). — Some little practice — not much — is needed for the regulation of breathing in this experiment; rather more, for the determination of the exact time at which exhaustion has set in. The following are typical results: those in brackets are taken from Aronsohn.

1. Crab-apple blossom	2 min. to 3 min.
2. White rose	1 min., 30 sec. to 2 min.
3. Heliotrope	4 min. to 6 min.
4. Ammonium sulphide	6 min. (4 to 5 min.)
5. Carbon disulphide	5 min. to 5 min., 30 sec.
6. Asafœtida	1 min., 30 sec. to 1 min., 55 sec.
7. Stale cheese	7 min., 45 sec. to 8 min., 30 sec.
8. Tincture of iodine	1 min., 30 sec. to 2 min., 15 sec. (4 min.)
9. Spirits of camphor	1 min., 45 sec. to 2 min., 45 sec.

Other results will be found in Aronsohn, p. 343. — In one test with white rose, *O* exhaled partly through the mouth, but partly also through the plugged nostril. The exhaustion time rose to 8 min., 10 sec. — Some odours are very irritating, and their irritation persists after the nose is exhausted for the scent. This is the case, *e.g.*, with eau de Cologne, and with the heliotrope of the above list. The limit of exhaustion is, in such cases, difficult to determine. — The carbon disulphide produces a drowsiness or dizziness, which is not at all unpleasant, but again makes the limit of exhaustion difficult to settle. — Practice

reduces the exhaustion time. The first two determinations for spirits of camphor (10 parts camphor, 70 alcohol, 20 water) were 5 min., 30 sec. and 7 min. These values resemble those of Aronsohn (5 to 7 min.) for 0.5 to 0.1 cc. camphor and 100 cc. of 0.6 % salt solution. After practice, the exhaustion times remain fairly constant.

EXPERIMENT (2). — Good scents for this purpose are:

(i) Nitrobenzole (nitrobenzene, essence of mirbane). This gives a momentary whiff of heliotrope. Then follows the scent of bitter almonds. After four or five breaths, the bitter-almond scent has altogether disappeared, and a scent is left which more or less resembles that of grey india-rubber tubing or benzine.

(ii) Benzoyl chloride. A very small quantity of this suggests, for the first few seconds, flower fragrance. The associations with the smell are usually indefinite: the smell is puzzling. Very soon there emerges simply a pronounced scent of bitter almonds.

(iii) Propionic acid. The scent of a trace of this liquid is a mixture (entirely unitary at first) of the scents of acetic acid and butyric acid. After a few breaths, the scent of acetic acid disappears, and only the unpleasant fatty-acid smell is left.

(iv) Faded violets. The scent is, again, 'mixed' at first, though quite unitary: it is a mixture of the scent of violets (as we have that scent in perfumes) with the scent of faded flowers. After a few breaths we have nothing but the repellent smell of faded flowers.

(v) Heliotropine. Heliotrope is smelled only for a few seconds; the following scent is that of bitter almonds. — Heliotropine probably contains nitrobenzole, *q.v.* A solution of heliotropine in odourless paraffin gives no final benzine scent.

(vi) Oil of camphor. For the first whiff or two, we have the scent of turpentine. This gives way to a nutmeg odour.

(vii) Oil of mace (solid) smells at first like nutmeg, and then like barn-yard manure. *Cf.* the peculiar odour of putrefaction which even freshly picked lilac blossoms give in large masses indoors.

(viii) Mutton tallow, if persistently smelled, yields an onion-like scent.

(ix) In practically every alcoholic solution, the smell of the alcohol comes out, at the expense of the original odour, if one smells it long. Even vanilla ice-cream, if it is over-flavoured, seems to 'taste of whiskey.'

The following variations of this experiment are worth making.

(i) Familiarise *O* with the smells of the aqueous solutions of coumarine and vanilline. — Mix the solutions in such proportions that only the vanilline can be smelled by *O*. Now let him exhaust his nostril for the *pure* vanilline solution. This done, let him smell the *mixed* solution. The liquid, which originally smelled only of vanilline, now smells only of coumarine.

(ii) Mix the aqueous solutions of coumarine, naphthaline and vanilline, in such proportions that at first smell the scent of the mixture is different from that of any one of the primary qualities. Let *O* smell the mixture continuously. Very soon an oscillation of qualities arises: some one of the components is smelled for a moment by itself, and then disappears to make way for another. Presently either naphthaline or coumarine goes out altogether, *i.e.*, ceases to appear in the oscillations. Then a second quality (coumarine or naphthaline) disappears, and only the vanilline is left.

The certainty and regularity of results in work of this kind are greatly increased if *O* is familiar, beforehand, with the scents which he is to smell out from the mixed odour: just as the hearing-out of an overtone from a clang is greatly facilitated by the separate sounding, beforehand, of that particular tonal quality. Hence it is well, after the first experiment has been made, to encourage *O* to identify, name, the partial scents that he has smelled; and then to repeat the test a few times over, in the light of this knowledge, taking the mean and the mean variation of the times of change.

If such identification is impossible, — and it is for some observers, scents being such intangible and elusive things, and things so little operated with in ordinary ideation, — the results of the experiment may be checked by noting (*a*) that the times of change still agree roughly for different observers, and (*b*) that the number of changes is constant, from one fairly practised observer to another.

EXPERIMENT (3). — The following are typical series:

TINCTURE OF IODINE.	SEC.	SEC.
Exhaustion time	115	98
" " after first 1 min. rest	60	60
" " " second 1 min. rest	50	38
" " " third 1 min. rest	38	23
" " " fourth 1 min. rest	29	22
" " " fifth 1 min. rest	37	12
" " " sixth 1 min. rest	37	11
" " " seventh 1 min. rest	33	11
" " " eighth 1 min. rest	30	5
" " " ninth 1 min. rest	21	
" " " tenth 1 min. rest	19	
" " " eleventh 1 min. rest	13	
" " " twelfth 1 min. rest	13	
" " " thirteenth 1 min. rest	8	

SPIRITS OF CAMPHOR.	SEC.	SEC.
Exhaustion time	105	114
" " after first 2 min. rest	69	68
" " " second 2 min. rest	52	50
" " " third 2 min. rest	46	48
" " " fourth 2 min. rest	38	34
" " " fifth 2 min. rest	46	47
" " " sixth 2 min. rest	32	20
" " " seventh 2 min. rest	34 (?)	16
" " " eighth 2 min. rest	22	12
" " " ninth 2 min. rest	25	12
" " " tenth 2 min. rest	17	12
" " " eleventh 2 min. rest	24 (?)	
" " " twelfth 2 min. rest	17	
" " " thirteenth 2 min. rest	17	
" " " fourteenth 2 min. rest	12	
" " " fifteenth 2 min. rest	8	

Others will be found in Aronsohn, pp. 344–346.

EXPERIMENT (4). — The following are typical results. The + indicates a positive rejudgment under the heading of its column (S = strong, W = weak, 0 = no scent). The asterisk indicates Aronsohn's results with the same stimuli (p. 347): where the identity of the stimulus is doubtful, the asterisk is enclosed in brackets. The obelisk indicates a few results, from another *O*, which differed from those of the proper *O* of the Table.

Stimulus.	Exhaustion by Iodine.			Exhaustion by Camphor.		
	S	W	0	S	W	0
Ol. petroselini	*+	—	—	+	—†	—
Coumarin	—	+	—	—	+	—
Ol. terebinth.	+	*†	—	—	+	—†
Ol. caryophyllorum	—	*+	—	+	—	—
Ol. cajeputi	*+	—	—	—	+	—
Flower perf. jasmine	+	—	—	—	+	—
Ol. rutæ	*—	+	—	—	+	—
Ol. pinus picea	—	+	—	—	+	—
Ol. lavand. gallic.	(*)+	—	—	—	+	—
Ol. bergamottæ	—	*+	—	—	+	—
Eau de Cologne	—	+	—	—	+	—
Ol. salviæ	+	*—	—	+	—	—
Ol. copaivæ	—	+	*—	—	+	—
Ol. juniperi	—	+	—	—	+	—
Ol. macidis	—	*+	—	+	—	—
Ol. fœniculi	*+	—	—	—	+	—
Ol. citri	—	*+	—	—	+	—
Musk (natur.)	—	+	—	+	—	—
Flower perf. neroli	—	+	—	+	—	—
Ol. aurant. dulc.	—	—	+	—	+	—
Heliotropine	—	—	+	—	—	+
Ol. anisi stellati	—	+	—	—	+	—
Fl. perf. ylang ylang	+	—	—	+	—	—
Ol. carvi	—	+	—	+	—	—
Fl. perf. hyacinth	—	+	—	+	—	—
Alcohol. 95 %	—	—	*+	—	—	+
Ol. succini	*+	—	—	+	—	—
Ol. rosmarini gallic.	(*)+	—	—	—	+	—
Ether	*+	—	—	—	+	—

The following Table shows, somewhat more fully, the results obtained from another *O*.

Stimulus.	Previous Judgment of *O* as to Intensity of Stimulus.	Exhaustion by Iodine.			Exhaustion by Camphor.		
		S	W	0	S	W	0
Ol. cajeputi	Moderate	* Strong	—	—	—	Very faint	—
Flower perf. jasmine	Heavy and strong	—	Faint	—	—	Faint	—
Ol. rutæ	Strong	*—	Moderate	—	—	Moderate	—
Ol. pinus picea	Weak	—	—	Nothing	—	Very faint	—
Ol. copaivæ	Rather strong	—	Rather weak	*—	—	Very weak	—
Ol. juniperi	Rather strong	—	Rather weak	—	—	Very weak	—
Ol. macidis	Weak and penetrating	—	* Very weak	—	Strong	—	—
Ol. fœniculi	Rather strong	* Rather strong	—	—	—	Weak	—
Ol. citri	Moderate	—	* Weaker	—	—	Rather weak	—
Musk (natur.)	Strong and heavy	—	Moderate	—	—	Still strong	—
Flower perf. neroli	Rather strong	Same	—	—	Same	—	—
Ol. aurant. dulc.	Moderate	—	*Very* weak	—	—	Weaker	—
Vanilline	Moderate	—	Weak	—	—	—	Nothing
Ol. succini	Strong	* Same	—	—	—	Moderate	—
Musk (artif.)	Weak	—	Very weak	—	—	Very weak	—
Ol. rosmarini gallic.	Strong	(*) Same	—	—	Same	—	—
Heliotropine	Weak	—	*Very* faint	—	—	—	Nothing
Ol. anisi stellati	Rather strong	—	Rather faint	—	Same	—	—
Flower perf. ylang ylang	Moderate	—	Faint	—	Same	—	—
Ol. carvi	Moderate	—	—	Nothing	Same	—	—
Flower perf. hyacinth	Strong	—	Moderate	—	Same	—	—
Ol. lavandulæ gallic.	Moderate	(*) Same	—	—	—	Weak	—
Ol. bergamottæ	Weak and sweet	—	* Weaker	—	—	Weaker	—
Eau de Cologne	Weak	—	—	Nothing	—	Very weak	—
Ol. salviæ	Strong	Same	*—	—	Same	—	—
Alcohol 95 %	—	—	—	* Nothing	—	—	Nothing
Ol. petroselini	Weak	* Weak	—	—	Weak	—	—
Ol. terebinth.	Moderate	—	* Weak	—	—	Weak	—
Ol. caryophyllorum	Strong	—	* Weak	—	Quite strong	—	—
Ether	Strong	* Strong	—	—	—	Not so strong	—

These two Tables do not agree in every instance, but they agree in the great majority of instances. We are fortunately

able to compare 16 of the results of each Table with results given by Aronsohn. The outcome is:

Table I.	Agreements	12	Disagreements	4
Table II.	"	13	"	3

It is noteworthy, further, (1) that the disagreements from Aronsohn are in no case extreme, *i.e.*, in no case fall outside of a neighbouring category, though they might have done so twice in Table I. and once in Table II.; and (2) that all three exceptions of Table II. are confirmed by the judgments of Table I.

The outcome of the experiment is that, during exhaustion of the organ for a given stimulus, a certain number of stimuli are still smelled at full intensity, certain others arouse a sensation of distinctly less intensity, and others again are not sensed at all.

The following materials (including odours from all Zwaardemaker's classes) are recommended for this experiment.

I. (a) Confectioners' 'pineapple oil.'
   (b) Beeswax.
   (c) Sulphuric ether.

II. (a) Rosemary, e. o.
   (b) α. Cloves, e. o.
       β. Cinnamon, e. o.
   (c) α. Anise, e. o.
       β. Peppermint, e. o.
       γ. Thyme, e. o. (according to Z).
   (d) α. Geranium, e. o.
       β. Bergamot, e. o.
   (e) Almond, e. o.

III. (a) α. Ylang ylang, β. Orange blossom; (b) α. Jonquille, β. Violet — the common perfumes.
   (c) Common benzoin, the liquid.

IV. (a) Oil of amber.
   (b) Musk (the tincture of natural animal musk, or the root).

V. (a) Carbon disulphide (much cheaper than allyl sulphide) or asafœtida.
   (b) A bit of strong dried fish, crumbled.
   (c) Dilute bromine, the alcoholic (?) tincture.

VI. (a) Creosote.
(b) Benzine.

VII. (a) Caproic acid or stale cheese.
(b) Root and stem of barberry or black currant.

VIII. (a) Laudanum.
(b) Olive oil, poured off from bed-bugs or squash-bugs.

IX. (a) Alcohol from half-decomposed vertebrate zoological specimens (not fish !), or water from wilted flower stems. Stapelia blossoms (carrion flowers) can also be used.
(b) Stinkhorns (Phallus impudicus) ; in alcohol, if necessary.

The author is unable, at present, to give the concentrations proper for quantitative work. In II. (d) *a* rose would be preferable to geranium, but is very expensive. Bergamot is less like geranium than citronella, and has a more marked scent than lemon. As for III., it seems to be almost impossible to procure in the American market a scent made from the real flowers; the pomades are more likely than the perfumes to approximate to the real flower odour. If one must use the 'chemical' compounds, there seems to be no good reason against taking those that are cheap and easy to get. On the one hand, if the common perfumes are dropped on cotton wool, the alcohol smell will disappear, and the flower odour will be left fairly pure. On the other, the De Laire specialties are (as was remarked above) overpoweringly intensive, and the proper concentrations are hard for a novice to work out. Moreover, their scent is apt to 'get all over' the laboratory.

The substances mentioned by Zwaardemaker under V. (b) have various drawbacks : some are poisonous, some very inflammable, some almost impossible to procure. For (c) bleaching powder and tincture of iodine may be tried. The bleaching powder has the advantage of eliminating the alcohol. Bromine seems, upon the whole, to be the least irritating of the three.

QUESTIONS.—(1) The result is, that the organ of smell can be entirely exhausted by an adequate stimulus, within the space of a few minutes ; and that an organ thus exhausted requires at least a minute for complete recovery. Both propositions hold for us, of course, only within the limits of our experiments.

The fact of exhaustion is no novelty. We find it, *e.g.*, playing a great part in temperature work on the skin, and in taste experiments on the fungiform papillæ. The completeness and the permanency of smell exhaustions are striking, because we are attacking the organ, as we think, in the gross, — whereas we are attacking but a single element of the skin or of the tongue. Really, however, as exp. (4) shows, we have not wearied the whole organ, but only some part or parts of it.

The reason for, or explanation of, the phenomena seems to be that they are not phenomena of 'exhaustion' at all, but phenomena of adaptation. Just as Hering's theory has substituted the concept of adaptation for that of retinal fatigue in the case of after-images, etc., so here it seems probable that we are in face of local adaptations of the organism to its environment, rather than of a mere giving-out under excessive stimulation. The teleological significance of such adaptation is obvious.

If we cling to an explanation in terms of exhaustion proper, we must reason that it is not the greater but the smaller liability to exhaustion of the sense-organs that has to be accounted for. The more primitive the organ, the more exhaustible would it be. Smell, taste and the temperature sense, developments from the 'chemical' sense of the lowest forms of life, still show phenomena of exhaustion; sight and hearing have come to possess a greater endurance, because they must be always 'on the stretch' for the avoidance of enemies.

If it be objected that certain animals use the sense of smell to find their food and their mates and to avoid their foes, the reply is that we do not know that their sense-organs are so readily exhausted as our own. The human organ may have regressed, from lack of use, to a primitive state of ready exhaustion. Note that Zwaardemaker denies the phylogenetic value of smell as a guardian of respiration (Phys. d. Ger., 10 f.).

(2) The mixed smells may be compared (*a*) to the mixtures of qualities from the black-white and colour series in vision (pinks, browns, etc.); (*b*) to the fusions of smell and taste which we have already noted; (*c*) to the 'heat' perception, obtained by the fusion of warm and cold; (*d*) to taste fusions, such as 'the taste of lemonade' (sweet and sour). There are probably other instances, but these are the most obvious. It should be noted that the sight fusions differ from the rest, in that we cannot get both the components of the mixture separately: we can get a

brightness quality without colour-tone, but never a colour-tone without brightness.

Colligation, a mutual enhancement of qualities by juxtaposition in space or time. *Cf.* contrast phenomena, rhythms, etc.; p. 419 below.

(3) The law is: that the end-organs of smell are possessed of specific energies, akin to the specific energies of the skin or the tongue, — but that, in all probability, these specific energies are not sharply differentiated from cell to cell, but distributed in zones of varying receptivity.

The first proposition is proved by the fact that complete exhaustion for one scent will leave other scents entirely unimpaired while certain others are as entirely obliterated. The second seems to be proved by the fact that many scents are weakened, but not obliterated, by exhaustion for a given scent. We must suppose that, while certain cells are, perhaps, quite specifically attuned to a single small quality, others are tuned to respond both to this specific quality, and, more weakly, to other qualities as well.

There is, it is true, an alternative view. The stimuli which we employ are in the great majority of cases complex, not simple. We might think, then, that the weakened scents are those which are made up in part of qualities identical with the qualities of the exhausting scent (these would not be smelled), and in part of different qualities (these would still be smelled). Only, in such an event, the scent that is weakly smelled ought to have changed, whereas there is no intrinsic reason for its being weakened. We ought to get, not the original odour, but certain selected components; and these might be strong. Observations on the point are difficult, and further work is needed. — See Zwaardemaker, Physiol. d. Geruchs, 277; Nagel, Zeits. f. Psych., xv., 86.

LITERATURE. — E. Aronsohn, Experimentelle Untersuchungen zur Physiologie des Geruchs, Archiv für [Anatomie und] Physiologie, 1886, 321 ff.

W. A. Nagel, Ueber Mischgerüche und die Componentengliederung des Geruchssinnes, Zeits. für Psych. u. Physiol. d. Sinnesorg., xv., 1897, 82 ff.

H. Zwaardemaker, Die Physiologie des Geruchs, 1895.

On the preparation of materials for the olfactometer, see the next Experiment.

EXPERIMENT XIX

§ 33. **The Olfactory Qualities.** (*a*) *Compensations.* — The principle of small compensation is familiar to us in everyday life. Most perfumes are used on the theory that they counteract unpleasant odours. Tooth-powder of orris root (Iris florentina) is employed to 'purify the breath,' *i.e.*, to kill the fœtor ex ore; and 'mint jujubes' appeal to smokers. Those who have lived in a house with a cat know the efficacy of burnt brown paper. Bridal bouquets often have gardenia mixed with their orange-blossoms, the aromatic scent weakening the too powerful balsamic odour. In medical practice, and in the operating room, recourse is had to the same principle.

MATERIALS. — The cylinders recommended are:

A Cedarwood (2) *d* *β*	*K* India rubber (ordinary red india-rubber tubing) (5) *a*
B Gum Benzoin (3) *c*	"
C Paraffin (the white wax of the histological laboratory) (7) *a*	"
D Beeswax (1) *b*	"
E Tolu balsam (3) *c*	"
E Tolu balsam (3) *c*	*D* Beeswax (1) *b*
D Beeswax (1) *b*	*C* Paraffin (7) *a*
B Gum benzoin (3) *c*	*F* Asafœtida (5) *a*
G Russian leather (tanned with sandalwood, not birch) (1) *c*	*K* India rubber (5) *a*
H Rosewood (2) *d* *β*	"

The numbers are those of Zwaardemaker's classes from which the scents are taken.

PRELIMINARIES. — For the construction of the cylinders, see E. A. McC. Gamble, Amer. Journal of Psychology, x., 1898, 32 ff. Begin their preparation in good time: they cannot be made in a hurry, and failures are numerous at first, however careful the operator. Keep every cylinder by itself, with a record of its use, in a self-sealing preserve jar.

The idiosyncrasies of the odorous substances must be learned by practice. The following account of india rubber may be helpful as an illustration.

(1) Choose a length of new rubber tubing, which gives a pure india-rubber smell. Old and stiff tubes are useless. (2) Be sure that the tube has never served to conduct any odorous gas or liquid. India rubber takes up very readily the scents of its surroundings: the passage of illuminating gas through a tube, for example, even for a few seconds, renders the tube worthless. (3) When the tube is not in use, keep a glass inhaling tube in it: otherwise the rubber will take up the scent of the drawer or closet in which it is kept. (4) India rubber hardens and loses its scent if exposed to the air. Hence it is unnecessary (though it is well, on principle) to put a paper cap on the cut end of the tubing. After the cylinder has been used a few times, this end will be quite odourless. (5) India rubber has the advantage that it can be smelled for a long time by most subjects without exhaustion of the organ. (6) The adhesion error is comparatively small. (7) If it is kept flexible, and free from contaminating scents, the tubing will retain its odour with undiminished intensity for years together. The little damage done to the superficial layers of the substance by exposure to the air during an experiment is made up, during the intervals of disuse, by diffusion from the deeper-lying layers. (8) India rubber is not affected by changes of temperature between the limits of 13° and 30° C. (9) Its odour, though not very strong, is very positive, — an odour not easily disguised by the presence of other smells. Hence it is well adapted for use in a general laboratory room, about which smells will inevitably hang, in spite of frequent ventilation. However, this property must not be presumed upon, or rule (2) will make itself felt.

The inhaling tube may be *cleaned* in various ways. The nose-piece should, of course, be thoroughly disinfected for each experiment. It may be dipped in a carbolic acid solution, or simply heated in the flame of an alcohol lamp. For washing out the tube, the student should be provided with a small funnel, two tins or cups for pouring and receiving water, absorbent cotton, a pliable brass wire for pushing the wad of cotton through the tube, a small alcohol lamp for drying, and (though this is not strictly necessary) some listerine. The listerine acts both as deodoriser and disinfectant, and its own scent is

easily washed away. See Zwaardemaker, Physiol. d. Geruchs, 104; Gamble, Amer. Journ. of Psych., x., 36 f.

The tube must be wiped as dry as possible inside and out, before it is held over the alcohol flame; else it will break. Indeed, the tubes break readily enough, with all the care that can be taken. A good supply must always be kept on hand.

Question (1) There are differences of opinion here, as there are differences of opinion regarding taste compensations (see p. 110). According to Zwaardemaker, all the substances recommended for this experiment are compensatory substances[1] (Physiol. d. Geruchs, 168, 268). Aronsohn's results, in spite of Zwaardemaker's interpretation of them, leave the question open (Physiol. d. Geruchs, 166, 267; Aronsohn, Arch. f. Physiol., 1886, 353; Nagel, Zeits. f. Psych., xv., 1897, 92). Nagel writes: "I have never observed complete compensation, though, in saying this, I do not at all mean to dispute its possibility" (Zeits. f. Psych., xv., 101).

In view of Zwaardemaker's numerical results, of the recognition of the principle of compensation in everyday life and in medical practice, and of his own experience, the author has no hesitation in accepting olfactory antagonism as a fact. This does not mean that every student can get a compensation effect in every experiment. We know that some of the well-meant attempts at the removal of unpleasant smells in ordinary life result in a mixture that is far worse than the original thing. The obtaining of a good compensation is partly a matter of luck, — of moving the cylinder at the right rate, catching the stimulus at the right moment of inhalation, etc.; and partly a matter of introspective alertness, — of pouncing on the no-smell moment with a confident and reliable judgment. If the observer is unable, after a fair trial, to indicate the compensation point on the scale, the experiment resolves itself into a determination of the two points, (*a*) at which the first smell is just overcome

[1] We can bear out this statement for all the substances but Russian leather (*cf.* also the result of the contrast experiments, p. 141). The discrepancy does not necessarily mean that Zwaardemaker is right, and that we are wrong, or *vice versa*. When one works in the rough, and not with chemically pure solutions, there is always a possibility of divergent results. *Cf.* Zwaardemaker, Arch. f. [Anat. und.] Physiol., 1900, 420.

by the second, and (b) at which the second, in turn, just gives way again to the first. Instead of saying, *e.g.*, that cylinders A and K compensate in the ratio 5.5 to 10, we should have to say that

6 cm. A and 10 cm. K give the A-scent alone, and
5 cm. A and 10 cm. K give the K-scent alone, —

so that the point of equality must lie somewhere between these limits.

The following are the compensation ratios, in cm., as given by Zwaardemaker:

$5.5\ A = 10\ K$	$7.0\ D = 10\ K$	$9.0\ E = 10\ D$
$3.5\ B =$ "	$7.0\ E =$ "	$5.0\ D = 10\ C$
$8.5\ C =$ "		

The following are typical laboratory results from two observers (double olfactometer):

10.0 cm. K compensates

In the direction K to A . . .	6.4 cm. A	5.6 cm. A
" " " " . . .	6.2 " "	6.0 " "
" " A to K . . .	5.0 " "	5.6 " "
" " " " . . .	4.4 " "	5.4 " "
Average . . .	5.5 ± 0.8 cm.	5.65 ± 0.15 cm.

10.0 cm. K compensates

In the direction K to B . . .	3.4 cm. B	4.0 cm. B
" " " " . . .	4.0 " "	4.4 " "
Average . . .	3.7 ± 0.3 cm.	4.2 ± 0.2 cm.

In these, and many similar cases, true compensations were found. That is to say, the odour, as the cylinder was moved to and fro about the point of equivalence, would be now A, now K, or what not: this oscillation would continue two or three times: and then, as the right point was struck, there would be a distinct and as it were positive nothingness of smell-sensation for an instant. The nothingness cannot be kept for more than an instant, but it can be refound without difficulty in another trial.

It will be noticed that the absolute values in cm. accord well with those of Zwaardemaker; this is not always the case, on account of the complexity and variability of conditions.

If the compensations of daily life, of gardening and of medical practice, are stable and permanent, why should not these experimental compensations be the same? A full answer to this question would involve reference to many conditioning factors: but a very simple consideration helps us out of the sort of difficulty which it raises for the student. The stable compensations are not nothingnesses, but partial compensations only. We still smell the orange-blossoms of the bouquet, but we smell them weakly, faintly. The point of exact compensation is not reached or, indeed, aimed at. Zwaardemaker says, *e.g.*, that in clinical practice "4 gr. iodoform and 200 mgr. Peruvian balsam appear *almost* odourless"; and that the surgeon "by spraying with carbolic acid can reduce, indeed, *almost completely* destroy, the stench of pulmonary gangrene" (Physiol. d. Geruchs, 165). The rough result is what is here wanted, not any extreme accuracy of balance.

(2) Zwaardemaker states that he sometimes obtained, in the near neighbourhood of the compensation point, a weak, indeterminate, but qualitatively simple impression, — a scent quite different from the two primary scents, and discoverable only by extreme attention (Physiol. d. Geruchs, 167; *cf.* Nagel, Zeits. f. Psych., xv., 90, 101). The author has found no trace of this mixed scent, in experiments with compensating substances; and it is difficult to see how its existence is reconcilable with the fact of compensation. Any case in which it is reported should be thoroughly investigated. — It is possible, of course, that this "schwache, undefinirbare Empfindung" (Arch. f. [Anat. u.] Physiol., 1900, 429) is identical with our "positive nothingness."

(3) In the first place, the inner cylinder may be contaminated; the air-current can get to it only by way of the outer cylinder, and the adhesion error comes in, in a new way. Secondly, the criticism is always possible that the odorous particles of the two substances combine, in some unknown physical or chemical way, to annul each others' smell-producing properties. We do not know enough to say, in physical or chemical terms, whether such criticism is just or not. The results of the second experiment throw it out of court as an argument against the fact of compensation.

(4) It would seem that olfactory compensation is a phenomenon of frequent occurrence and of high importance. We know too little, as yet, to draw any definite conclusions from the facts: besides which, the facts themselves, as we have seen, are by no means universally admitted. They carry with them, however, the suggestion that the ultimate specific energies of smell must be pretty numerous: many more than the six of sight or the four of taste. Zwaardemaker meets the suggestion by assuming a serial differentiation (a *skalenbildende Schattierung*) of scents within each of the specific-energy groups (Physiol. d. Geruchs, 268). More recently, viewing the facts in the light of a general theory of mental inhibition, he says that "compensations are to be expected in all cases where there are even partial differences of quality," and draws a parallel between intensive odours and unsaturated colours (Arch. f. [Anat. u.] Physiol., 1900, 432).

(*b*) *Mixtures.* — The principle of smell mixture is, also, familiar to us outside of the laboratory. The flower-scents of the perfume industry, *e.g.*, are in many cases quite complicated mixtures. Thus the scent of 'heliotrope' is obtained from a mixture of vanilla, rose, orange-flower, amber and almond.

MATERIALS. — The series of scents employed should contain members drawn from each of Zwaardemaker's classes: *cf.* the list recommended on p. 127. The results quoted below (p. 137) were, unfortunately, not gained by systematic work of this kind.

QUESTIONS. — (1) Again, we find difference of opinion in the literature. Nagel writes: "I have never found a pair of odorous substances that did not give a mixed scent [*i.e.*, a true smell mixture] when compounded at the right relative intensities" (Zeits. f. Psych., xv., 95). Zwaardemaker admits the fact of mixture, but only as between scents of the same or of closely related classes (Physiol. d. Geruchs, 280). Aronsohn finds mixture in some cases, rivalry [alternation of the two original qualities] in others; though he says that, if more than two components are taken, a mixed scent is always produced (Arch. f. Physiol., 1886, 353). Valentin declares that two simultaneous smell-stimuli, of approximately equal intensity, set up, for him, two simultaneous but distinct sensations: precisely the effect

hich Nagel denies (Lehrbuch d. Physiol. d. Menschen, 1844, ., 2, 292, § 4119). What is wanted, then, is systematic work ith different intensities of the various qualitative stimuli.

Those who obtained the compensation result in the foregoing xperiment will require no arguments to prove that certain cents do *not* mix. The rule with the compensative scents is: scillation, nothing. Those, on the other hand, who failed to ecure true compensations may incline to agree with Nagel. Ievertheless, direct experiments rarely if ever fail to convince he observer that there are some scents which do not mix, as ell as very many which do. — Criticism of these experiments ill be given below, under (4). The following are typical results·

ixtures. —

Musk (artif.) and Opium	Classes	(4) *b*,	(8) *a*.
" Vanilline	"	"	(3) *c*.
" Listerine	"	"	(2)
Musk (nat.) and Pyridine	"	"	(6) *a*.
Caryophylline and Ol. carvi	"	(2) *ba*,	(2) *ca*.
Iodine and Ylang ylang	"	(5) *c*,	(3) *aa*.
" Camphor	"	"	(2) *a*.
Camphor and Ol. aurant. dulc.	"	(2) *a*,	(2) *dβ*.
" Ol. anisi	"	"	(2) *ca*.
" Ylang ylang	"	"	(3) *aa*.
" Laudanum	"	"	(8) *a*.
" Ol. terebinth.	"	"	(2) *a*.
" Ol. juniperi	"	"	(2) *ca*.
Laudanum and Ol. juniperi	"	(8) *a*,	(2) *ca*.
" Allyl sulphide	"	"	(5) *a*.
Iodine and Ol. succini	"	(5) *c*,	(4) *a*.
Valerianic acid and Ol. lavandulae	"	(2) *cγ*,	(2) *cγ*.
" Ol. salviae	"	"	(4) *a*.
" Hyacinth	"	"	(3) *ba*.
Petroleum and Ol. salviae	"	(6) *b*,	(4) *a*.

o mixture. —

Pyridine and Jasmine	"	(6) *a*,	(3) *aa*.
" Hyacinth	"	"	(3) *ba*.
Valerianic acid and Heliotropine	"	(2) *cγ*,	(3) *c*.
Opium and Ol. salviae	"	(8) *a*,	(4) *a*.
Neroli and Petroleum	"	(3) *aβ*,	(6) *b*.
Camphor and Allyl sulphide	"	(2) *a*,	(5) *a*.
" Petroleum	"	"	(6) *b*.

These results (1) speak for Nagel and against Zwaardemaker, on the point that members of remote classes may mix as readily as members of nearly related classes (Physiol. d. Geruchs, 283 f.; Zeits. f. Psych., xv., 94); while (2) they speak for Zwaardemaker and against Nagel, on the point that there are certain scents which do not mix at all, but persist side by side in olfactory rivalry.

(2) The resultant scents differ very greatly as regards permanence. Some are quite stable; others as fleeting as the nothingnesses of the compensation experiments. The 'mixtures' of the list just given are stable. On the other hand, instable mixtures were found as follows:

Allyl sulphide and Musk (nat.)	Classes	(5) *a*,	(4) *b*.
Heliotropine and Musk (artif.)	"	(3) *c*,	(4) *b*.
Citral and Ol. carvi	"	(2) *dβ*,	(2) *ca*.
Camphor and Ol. citri	"	(2) *a*,	(2) *dβ*.
Iodine and Ol. salviae	"	(5) *c*,	(4) *a*.
Valerianic acid and Camphor	"	(2) *cγ*,	(2) *a*.

For other instances, and explanation, see Nagel, Zeits. f. Psych., xv., 95.

(3) Yes: and similar evidence was afforded by the compensation phenomena.

(4) The experiments above quoted, and the whole Experiment as described in the text, are always open to the objection that they do not control the *intensities* of the stimuli employed. Nagel is quite right in demanding "the right ratio of intensities of the two qualities" (p. 92): and his own conclusion, that all odorous substances mix for sensation, is limited by the phrase "bei geeigneter Intensitätsbemessung" (p. 95). We ought, then, if we are to settle the point, (1) to employ scents from all of Zwaardemaker's classes, and (2) to employ them in the form of olfactometric cylinders, so that we can mix all intensities (at least, all intensities from liminal to very strong) of all stimuli. The student may be left to devise a regular method of procedure, on the lines of rules already laid down.

On the other hand, no amount of control of intensities can do away with the compensation results on the olfactometer; and the fact that some mixtures are stable, and others curiously transient and elusive, suggests at least the possibility that these latter may be terms in a series which ends with complete refusal to mix.

The experiment may be continued, if the Instructor deem it advisable, with more than two stimuli. Points to notice are: (1) that increase of the number of components means, as a general rule, a more penetrating and permanent smell mixture; but (2) that the intensity of the resulting mixture is, also as a rule, noticeably less than the intensity of the strongest component. — Nagel, pp. 99, 101; Zwaardemaker, Physiol. d. Geruchs, 167.

(*c*) *Contrasts.* — Very little is said of smell contrast in the literature. Zwaardemaker cites Linnæus to the effect that the scent of musk contrasts with fæcal smells; and remarks himself that "cheese and Bordeaux, high game and Burgundy, are evidently opposed odours" (Physiol. d. Geruchs, 251). It is still uncertain whether taste contrasts are comparable to visual contrasts, and the antagonisms in taste and smell to the three antagonisms of sight. It is uncertain, too, whether smell mixtures are more nearly akin to mixed colours or to the fusion of tones in a clang. But it is as difficult to suppose that there is no contrast, where there is antagonism, as it is to think that antagonistic processes can give rise to a mixed sensation (see p. 135). And the argument from analogy seems to be fully borne out by experiment.

MATERIALS. — The cylinders are:

A Cedarwood (2) *dβ*
B Gum benzoin (3) *c*
D Beeswax (1) *b*
E Tolu balsam (3) *c*
G Russian leather (1) *c*
K India rubber (5) *a*
L Glycerine soap (1) *c* and (7) *a*
M Cocoa butter (2) *b* ? and (7) *a*

Weak glycerine soap gives a pure ethereal smell; strong, the ethereal mixed with a fatty smell. Very weak cocoa butter is merely fatty; stronger, fatty and aromatic. For the construction and properties of the new cylinders, see Amer. Journ. of Psych., x., 32 ff.

PRELIMINARIES. — The meaning of the 'stimulus limen' must be explained to the student. See Külpe, Outlines of Psychology, 34; Titchener, Outline, 78 ff.

The method given for the determination of the limen is very incomplete. Its incompleteness does not, however, affect the relative value of the experimental results. Külpe gives the method in full (Method of Just Noticeable Stimuli: pp. 55 f.); and it may be explained to the student, and the method of the text modified, if the Instructor wishes (see Question 2). We shall describe the complete method in vol. ii.

There may arise, with some *O*'s, during the preliminary work upon the K-limen, a tendency to determine the liminal value in terms not of smell but of hand-movement. The limen gets to be thought of, unwittingly, not as a just noticeable smell, but as a push-out of the cylinder just so far. The error is not by any means general. As a rule, too, both *E* and *O*, if they are working conscientiously, will discover and report the tendency. But if the Instructor suspect the error, he should question *E* and *O* (separately) for evidence of it, and in further work let *E* move the cylinder, telling *O* that it will not be moved at a uniform rate.

If the question of this source of error comes up, let *O* and *E* face it, and work out the possible influence of the error in contrast experiments.

EXPERIMENT. — Great care must be taken that the adhesion error is not neglected.

The following results are typical:

O (1):	Limen for *K*,[1]	10	experiments,		7.0 ± 0.6 mm.
O (2):	"	"	"	"	6.8 ± 0.4 mm.

[1] The *K* of these results is *black* (not red) rubber tubing.

	Cylinders.	Limen for *O* (1).	Limen for *O* (2).
I.	*K*	6.0 mm.	5.0 mm.
	K after *A*	2.0	3.0
	K	6.0	6.0
	K after *E*	3.0	3.0
	K	6.0	6.0
	K after *D*	4.0	4.0
	K	6.0	5.0
	K after *A*	2.0	2.0
II.	*K*	7.5	8.0
	K after *D*	6.0	6.0
	K	10.0	7.5
	K after *B*	8.0	5.0
	K	0.0	10.0
	K after *E*	6.0	7.0
	K	8.0	10.0
	K after *B*	6.0	6.0
III.	*K*	5.0	6.0
	K after *L*	5.0	6.0
	K	6.0	5.0
	K after *M*	8.0	5.0
	K	7.0	7.0
	K after *G*	8.0	8.0
	K	6.0	6.0
	K after *M*	7.0	6.0
	K	6.0	6.0
	K after *L*	8.0	7.0
	K	6.0	6.0
	K after *G*	7.0	7 0

evident that there is always a slight reduction of the limen previous stimulation by a compensating stimulus, whereas ılation by glycerine soap, Russian leather and cocoa butter r leaves the limen as it was or slightly raises it. — The different series were taken at three days' intervals, I. .e early morning, II. and III. in the afternoon. Neither ew the other's results. Both were well trained in olfactory

.

JESTIONS. — *E* (1) There is good evidence of contrast, in ts like the above. This evidence tallies with the results of

the compensation experiments: *cf.* the Russian leather result, p. 133.

E and *O* (2) The students should be encouraged to think out a method, giving the reasons for every step. The principle of the Method of Just Noticeable Stimuli may very well occur to them.

E and *O* (3) The contrast-stimulus might be given to the one nostril, and the following K-stimulus to the other. The following results were gained, under these conditions, by the two *O*'s already quoted:

Cylinders.	Limen for *O* (1).	Limen for *O* (2).
K	7.0 mm.	6.0 mm.
K after *A*	4.0	4.0
K	7.0	6.0
K after *D*	5.0	4.0
K	6.0	5.0
K after *B*	3.0	3.0

INSTRUMENTS. — Fig. 15 shows the principle of Zwaardemaker's Fluid-Mantle Olfactometer: *a*, odorous liquid; *b*, kaolin cylinder; *c*, screen; *d*, inhaling tube. A double fluid-mantle olfactometer, made entirely of metal, is figured in L'Année psychologique, v., 1899, 215; it is supplied by the mechanician of the Physiological Laboratory, Utrecht, for fl. 48. A single fluid-mantle olfactometer costs fl. 32.

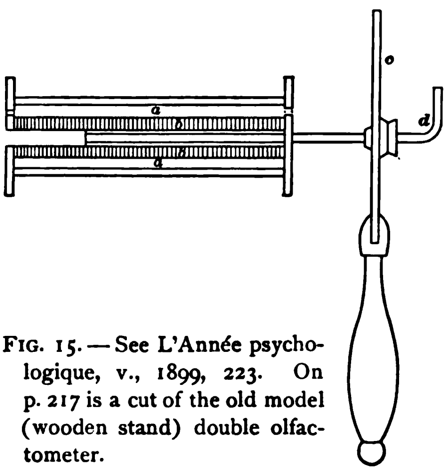


FIG. 15. — See L'Année psychologique, v., 1899, 223. On p. 217 is a cut of the old model (wooden stand) double olfactometer.

CHAPTER VI

Organic Sensation

§ 34. **Organic Sensation.** — On organic sensations, see A. Goldscheider, Ges. Abh., ii., 1898; Külpe, Outlines, 140, 146; G. E. Müller and F. Schumann, Pflüger's Arch., xlv., 1889, 37; E. B. Delabarre, Ueber Bewegungsempfindungen, 1891; E. Kröner, Das körperliche Gefühl, 1887; E. Mach, Grundlinien d. Lehre von d. Bewegungsempfindungen, 1875; Foster, Text-book of Physiol., iv., 1891, 1433; C. Richet, Recherches expérimentales et cliniques sur la sensibilité, 1877; E. H. Weber, *op. cit.*; Wundt, Phys. Psych., i., 1893, 419; Sanford, Course, 25, exps. 33–51; O. Funke, *op. cit.*; H. Beaunis, Les sensations internes, 1889.

The literature of the static sense is large. An appreciation of theories, and the devising of an inexpensive instrument for ordinary laboratory work, might be assigned, as a minor problem, to an interested student.

Preliminary Exercises. — (1) See Külpe, 142; Titchener, Outline, 68 f.

(2) Goldscheider, 323. The student may also perform the experiments on the 'paradoxical' sensations of resistance and weight, 90 ff.

Questions. — (1) Külpe, 140.

(2) Titchener, Outline, 50 f.

(3) The points to be emphasised are (*a*) the importance of the sensations for the psychological 'self,' (*b*) their importance as the vehicle of the sense-feelings and (*c*) the part played by them in recognition, recollection, etc.

EXPERIMENT XX

§ 35. **The Sensation of Muscular Contraction.** — The term 'muscular sense' has been badly abused in psychological literature. It has come to mean "l'ensemble des sensations qui

nous renseignent sur l'état de nos organes moteurs" (Henri), the sense of vision being by tacit consent excluded from the definition. Henri, who gives a good Revue générale sur le sens musculaire (in this wider signification) in the Année psychologique, v., 1899, 399 ff., admits that "le terme est très mauvais," but thinks it the best of existing titles for his subject. There seems, as a matter of fact, to be no reason whatever for continuing a usage which originated in a false theory (the theory that muscular sensations proper give us our perceptions of movement) and which now requires a prefatory apology from the writer. If we wish to group together the sensations in question, to emphasise their community of function, we may employ Bastian's phrase, 'kinæsthetic sensations.' If we are working analytically, it is best — indeed, it is the only justifiable course — to speak of 'muscular sensation,' 'tendinous sensation,' 'articular sensation,' as we speak of sight, hearing, cutaneous sensation, etc. The sensation which we isolate in the present experiment is the sensation whose peripheral end-organ is striped muscle, and whose stimulus is muscular contraction: just as a visual sensation is a sensation whose peripheral end-organ is the retina, and which is set up by the action of light (or some inadequate stimulus) upon the retina.

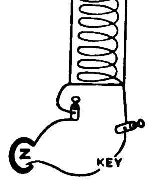


FIG. 16. — Arrangement of inductorium for single (unmodified) shocks. A. Waller, An Introduction to Human Physiology, 1891, 315. *Cf.* Fig. 11.

MATERIALS. — The student should familiarise himself with the theory and use of the physiological inductorium. Good explanatory diagrams are given by Waller, Human Physiology, 315. Fig. 16 shows the connections required by the present experiment. The Helmholtz side-wire is removed, and the Wagner hammer (interrupter) is out of function.

The form of weight employed, key, cells, arm-rest and electrodes may of course vary considerably, without variation of any of the essential requirements of the experiment.

PRELIMINARIES. — The introspective record (which may be dictated to *E*, or written by *O* with his right hand) will run somewhat as follows.

LIGHT PRESSURE. — 'Pricking, tickling sensations, due to the roughness of the chamois skin. Pressure from the skin. Shape and relative size of the stimulus clearly perceived. Tendency to rub or scratch the arm after removal of stimulus. No subcutaneous sensations.'

HEAVY PRESSURE. — 'Tickling much less. A deeper skin sensation, as if from the under part of the skin. The roughness and coolness of the first application change to smoothness and warmth. Towards the end of the time of stimulation, very little sensation at all; what there is, is definitely localised in the skin.'

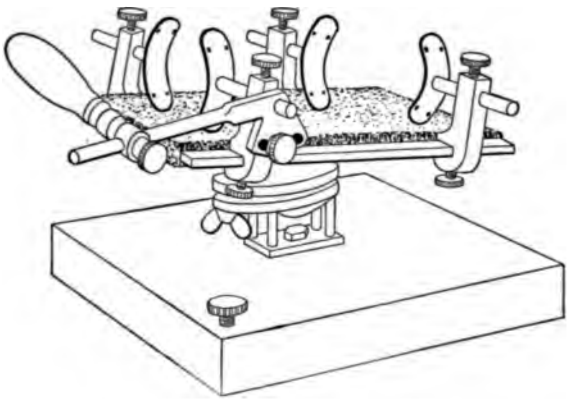


FIG. 17. — Arm-rest, designed for use with kinesimeter; Fig. 9, above.

EXPERIMENT (1). — A. Goldscheider, to whom we owe this experiment, advises (1888) a subcutaneous cocaine injection for the anæsthetising of skin, subcutaneous tissue and the superficial layers of the muscle under investigation. Physicians are now strongly of the opinion that cocaine injections should be avoided, unless the patient's constitution is thoroughly known, and that in any case the cocaine should be injected only in very small quantities. Ether spraying is safe, and answers every purpose. If long continued, it anæsthetises not only the skin, but the subcutaneous tissue as well.

The introspective record will be, in brief, somewhat as follows. 'The ether must be applied several times before there is any loss of the sensations above described. Presently, the light,

tickling, pricking sensations become indistinct, and a dull, diffuse sensation takes their place. This is not localised in the skin, but just beneath it. It is shallow as regards the muscle, but decidedly beneath the skin.

'After further etherisation, the cutaneous sensations disappear entirely. Nothing can be said of the temperature, pressure, size, shape, smoothness or roughness of the stimulus. The dull sensation has gone deeper into the muscle, and is more vague and indefinite than before. There seems to be a hard, dead lump in the muscle; and at times it is as if the muscle fibres were ground or rolled against each other. The diffuse and indefinite, and yet definitely "inside" sensation persists from now on; it seems to be quite simple, but is exceedingly difficult to describe.

'No change occurs, as the spraying is continued, except that the muscle sensation becomes duller and more indefinite, and perhaps deeper seated. It is like the feeling of a tired, overworked limb. In general, it has not the vigour or freshness of cutaneous sensations, though now and again it is almost a pain.

'After the spraying has ceased, the sensations come back by degrees, in the order from within outwards. The change seems to be one from simplicity to complexity, and also from vagueness to distinctness and acuteness.'

It is hardly possible to get nearer to the specific quality of the muscle sensation than these analogical and descriptive phrases take us. Goldscheider's words are: "eine in der Tiefe localisirte dumpfe Empfindung von eigenthümlichen Charakter"; "das Gefühl ist in der Tiefe localisirt und ist diffuser Art"; "diese dumpfe, ziehende Sensation in dem Gebiete des Muskels"; "man kann die Muskelempfindung bis zum Schmerz steigern": expressions that tally well with the introspective records just quoted.

Experiment (2).—On the determination of the 'motor point,' see, *e.g.*, W. Erb, Handbook of Electrotherapeutics, trans. by L. Putzel [1883], 121 ff.

Goldscheider notes that currents which produce a slight, though distinctly visible muscular contraction, give no muscular sensation at all; the contraction must be sharp and strong, if

the sensation is to result. The student will be able to verify this observation as he gradually moves the secondary in towards the primary coil.

The result of electrical stimulation accords with that of the foregoing experiment. 'The sensations are as before, only not so definite. There seems to be a difference in intensity, but the sensations themselves are exactly alike. The muscle sensation is again vague, hard, indefinite, dull, dead, at times almost painful; it seems to be seated deep down in the muscle.' The difference of intensity can be eliminated, if *O* desires, by moving the secondary still nearer to the primary coil.

The involuntary contraction of the finger plays but a very small part in consciousness during the experiment. 'I seemed to be helpless to prevent the twitch; it came from outside, and was something that I was not concerned with. I had a slight, sharp sensation in the finger tip, but I did not pay enough attention to it to recall anything of its nature.'

EXPERIMENT (3). — The same dull, diffuse, hard sensation can readily be identified as the mechanical pressure is increased.

ADDITIONAL EXPERIMENT. — Let *O* fatigue his arm by means of the finger dynamometer prescribed for Experiment XXIII., and attempt to analyse out from the fatigue complex the peculiar quality of muscular sensation which he has identified in the above experiments.

QUESTIONS. — (1) There can be no doubt as to the specificity of the muscular sensation: Goldscheider says unhesitatingly that it "sich sehr merklich von jeder anderen Empfindung unterscheidet." It has, nevertheless, a decided resemblance to the sensation of cutaneous pressure; such a sensation, *e.g.*, as is set up by the pressure of the blunt compass-points in æsthesiometric work (see Experiment XXXIV.). The author was for a long time inclined to regard these two sensations as qualitatively indistinguishable, and to refer their differences of diffusion, distinctness, etc., to associated processes. Recent work has, however, assured him that the 'dulness,' 'deadness,' 'diffuseness' of the muscular sensation constitute it a new quality. There is, probably, no better name for it than the sensation of 'muscular pressure.'

(2) "In all transformers, the electromotive forces generated in the secondary circuit are to those employed in the primary circuit nearly in the same proportion as the relative numbers of turns in the two coils. For example, if the primary coil has 100 turns, and the secondary has 2,500 turns, the electromotive force in the secondary circuit will be nearly 25 times as great as that used in the primary" (S. P. Thompson, Elementary Lessons in Electricity and Magnetism, 1895, 218). This is the principle of the inductorium. The primary coil has comparatively few turns of thick wire: the pressure (volts) is low, the current (amperes) relatively heavy. The secondary coil has a great many turns of very fine wire: the pressure is enormously increased, so that we can send the current through the resistance offered by living tissue; the current itself, on the other hand, is so light as to be entirely innocuous. — The student should understand the difference between the make and break currents, and the reasons for the greater suddenness and sharpness of the latter.

LITERATURE. — See, besides the passages cited from Henri, Waller and Thompson, A. Goldscheider, Gesammelte Abhandlungen, ii., 1898, 37 ff.; H. C. Bastian, The Brain as an Organ of Mind, 3d ed., 1885, 543; W. Erb, Handbook of Electrotherapeutics [1883], 15, 125.

CHAPTER VII

THE AFFECTIVE QUALITIES

§ 36. **Affection.** — The unsettled state of the psychology of the affective processes is something of a scandal to experimental psychology, although excuses for it are not far to seek. (1) The processes themselves are notoriously elusive, disappearing as we try to attend to them, and translating themselves into ideas at the slightest possible provocation. (2) We get no direct aid from physiology. There is no peripheral organ, as there is in the case of sensation; and the central conditions of feeling are simply matters of speculation. (3) It is true that we get indirect aid from physiology, in the form of the 'method of expression.' The curves of breathing and volume and pulse doubtless tally with the variations and trends of affective consciousness. But until we know the affective consciousness itself, how are we to be sure of interpreting our curves? One has not to go far into the literature to find what one is tempted to call ridiculously different readings of the same curve. (4) As for psychological methods, to supplement the physiological, we have only (*a*) the method of impression, which holds out no promise of ever settling the question of the number of affective qualities, and (*b*) the method of suggestive disintegration, which attempts to analyse out each of these qualities by suggesting the others away, — itself at present rather a crude suggestion of method than a practicable rule of working. (5) Corresponding to this dearth of settled facts, we have a hypertrophy of theory and a large controversial literature. Partly because the theories are intrinsically interesting, and partly because of the sheer bulk of the literature, the fundamental issues of affective psychology are apt to be left out of sight. It is easier and more exciting to criticise so-and-so's theory of pleasure-pain, than to face the problem of the affective qualities for oneself; and, indeed, theory has had

so wide a range that one can hardly turn in any direction without being confronted by some so-and-so's speculations.

The author has taken up a conservative position in the text, not because he is enamoured of it as a position, — James' theory of emotion and Wundt's theory of the affective qualities are far more picturesque, — but because he believes that it offers the safest ground from which to work towards a better affective psychology. And for that we must look (1) to physiological advance, to increased knowledge of what Fechner called 'internal psychophysics,' and to increased knowledge of the *physiological* basis of our curve variations, and (2) to the gradual emergence of an introspective consensus. This last is not entirely hopeless, seeing that introspection is constantly sharpening and refining, under the influence of the experimental method at large.

For the various theories mentioned in the text, the following books may be consulted:

(1) (*a*) Th. Ziehen, Introd. to Phys. Psych. (trans. by C. C. van Liew and O. W. Beyer), 1895, 130; Leitfaden, 5th ed., 1900, 108.

(*b*) C. Lange, Ueber Gemüthsbewegungen, eine psychophysiologische Studie, 1887, 76.

(*c*) H. Münsterberg, Beitr., iv., 1892, 216. The James theory of emotion (Psych., ii., 449 ff.; *cf.* articles in Mind, Psych. Rev., Phil. Rev., Rev. phil., etc.) has points of resemblance both to the Lange and to the Münsterberg hypothesis.

(2) W. Wundt, Outlines of Psych., 1897, 74; Vorlesungen üb. Menschen- und Thierseele, 3 Aufl., 1897, 239; Philos. Studien, xv., 1899, 149; Völkerpsych., i., 1, 1900, 37 ff.

The common theoretical basis of (1) is the reduction of mind to sensation elements. The theoretical basis of (2) seems to be the thought that, because every emotive attitude is 'unique,' therefore the simplest characteristic processes of all emotions must be unique. Now the major premiss in this argument is, in a certain sense, true: the feeling of moral obligation, the pride in the birth of your first baby, the satisfaction in a new dining-room carpet, your emotive experience under the Ninth Symphony, these are all 'unique' consciousnesses, each specifi-

cally different from all the rest, none reducible to any one of the others. But (and this is the point) is it because they are differently put together; because, while their elements are the same, the elements are differently selected, proportioned, arranged; that they are termed unique? Or is it because they contain elements ultimately different in kind? By all analogy, the former hypothesis is the first to be tested, and the latter is to be considered only if the first utterly breaks down.

For the theory here adopted, see Külpe, Outlines, 225 ff.; A. Lehmann, Die Hauptgesetze d. menschl. Gefühlslebens, 1892, 12 ff., 75 ff.

EXPERIMENT XXI

§ 37. **The Affective Qualities: Paired Comparison.** *Cautions not noted in the Text.* — It must be carefully impressed upon the student that the affective values of the curve obtained in this experiment are relative and not absolute. A colour may be judged to be more pleasant than 26 other colours, and yet may be, intrinsically, very weakly pleasant. The point may be clinched by showing *O*, at the conclusion of the tests, that colour which has been most often preferred, and asking him to gauge its pleasantness by that of a good dinner, or a brisk walk, or a cool drink when thirsty, or the scent of a flower. In comparison with these and many other sources of sense pleasure, the colour patch will seem practically indifferent.

If *O* promises to be very fertile in association, a short preliminary series of comparisons may be taken, before the experimental series begins. The associations soon lapse, when *O* finds that they are only hindering him, and when the experiment has become a matter of routine. — On no account must *O* suppose that the experiment is a test of æsthetic taste; that he 'ought' to like certain colours in combination, and dislike others. He must be assured that every judgment, no matter what it is, is on precisely the same level of value with every other: it is the judgment that is recorded, not the æsthetic rightness or wrongness of the judgment. The more passive and, so to speak, mechanical he can be in face of the stimuli, the better.

The required passive attention to the colour impressions, and the affective reaction upon them, are incompatible with discur-

sive introspection. Introspection implies an active attention to a sense contents. Although, therefore, *O* may be questioned, after the experiment, about the sense factors in his judgments (brightness, saturation, contrast, effect of frame and contents as a whole *vs.* effect of separate colour squares, influence of succession of colours, etc.), and although any remarks of the kind that he may volunteer should be noted down by *E*, yet he should by no means be encouraged to think about the experimental conditions or appliances. He has passively to *live* the colour, to *be* the colour; and then, before intellectual processes have time to start up, to name his affective response to it.

QUESTIONS. — (1) The curve will certainly show a preference, if the work has been properly done. As to the preference itself: there seem to be two types of *O*. The one prefers saturated colours, — and this type probably constitutes the majority; the other as definitely prefers unsaturated, what have come to be called in the popular phrase 'artistic' colours. The two curves printed herewith are taken from *O*'s of the first type. — No rule can be laid down, it seems, as regards preference for individual colours. In the two curves here given, the maxima and minima are very near together, as indeed the whole curves are greatly alike. On the other hand, many observers of this type have a great aversion to yellow, which here stands high. We are, genetically, so far remote from an intensive affective reaction to simple colour patches, that such differences must be expected.

(2) The answer to this Question cannot be foreseen. It is, perhaps, safe to say that as a rule *O* is *not* aware of his preferences. He may know vaguely that he likes 'rich' colours better than 'poor'; or he may have a single and intense liking for a certain colour as seen against a certain background, *e.g.*, yellow upon black, or blue upon grey: but it is doubtful whether he can, even approximately, construct his curve for himself by introspection. Such, at least, has been the author's experience. — The reason would be the same as the reason for the vagaries of preference just noted, under (1).

(3) It is, again, a nice point whether the colours have any emotive value, apart from their associations. *Is* R an angry

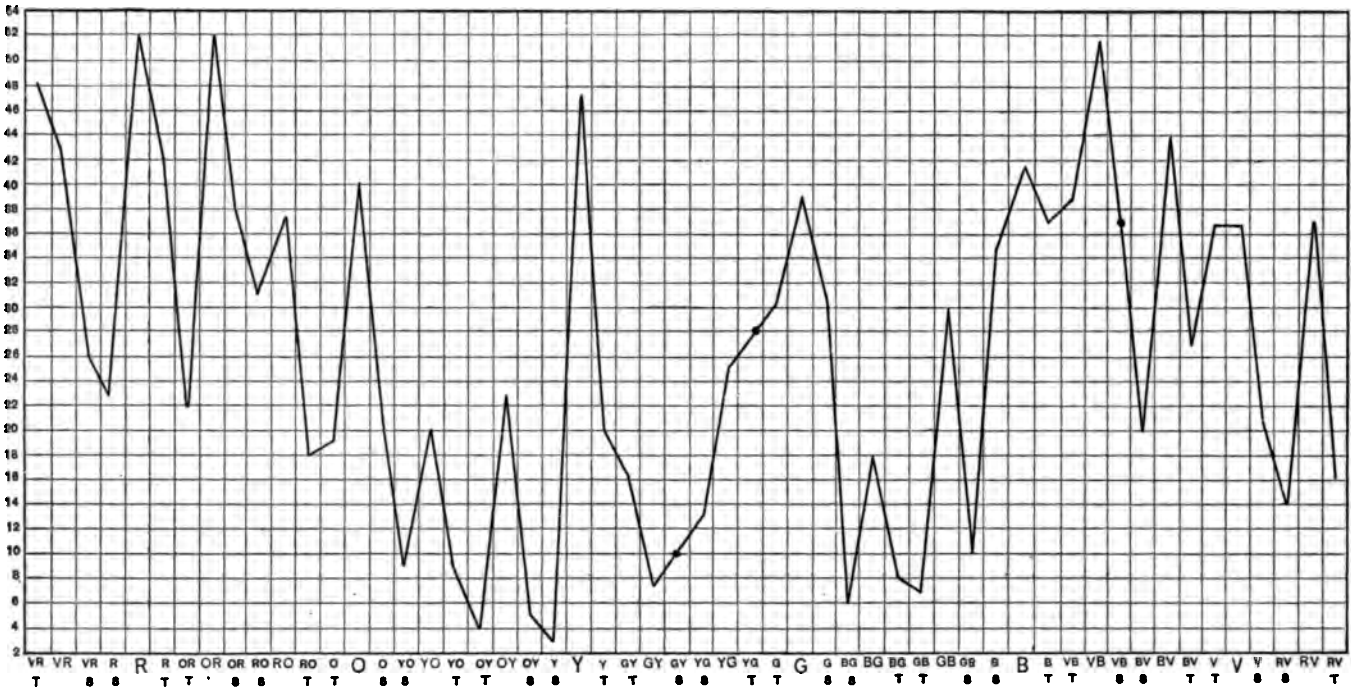


FIG. 18.—Curve of affective reaction to colours (method of paired comparisons). Ordinates: number of preferences. Abscissæ: Milton Bradley coloured papers; T = tint, S = shade.

colour, and G a hopeful colour, and B a depressing colour? Or are these things associations merely, — blood or the flushed face, growing vegetation, the steel-blue of a lowering sky? It is hardly profitable to speculate. It is, however, worth while to note whether *O* appears (on the successive experimental days) to judge the colours according to his mood, or to have a mood impressed upon him by the first few pairs of colours. *E* should note down all indications. In the author's experience, the former alternative is realised.

In later affective work, we shall see reason to be very careful that *O*'s mood, at the beginning of the experiment, is indifferent. Here indifference is not required. For even if *O* be in a mood to dislike everything, he will still dislike some things less than others; and if he does that, his judgments are valid for our purpose. Of course, a mood of steady indifference is favourable to the mechanising of the whole procedure, which we said above (p. 152) was desirable.

(4) Tastes and smells. For the psychological reasons, see Outline of Psychology, 225 ff. — Tastes could hardly be worked with, as there are so few taste qualities. Smells have enough variety, but are extremely and insistently associative. However, it would be well worth while to apply the method of paired comparisons to them.

LITERATURE. — J. Cohn, Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten und ihrer Combinationen. Philos. Stud., x., 1894, 562 ff.

§ 38. **Alternative Experiment.** — We may employ the Method of Impression in another form, substituting 'serial judgments' for 'judgments by paired comparison.'

MATERIALS. — Set of coloured paper squares, 7 by 7 cm. Piece of neutral grey (or black, or white) cardboard, 60 by 60 cm., having at the centre a window 6 cm. square. Cross-ruled paper.

PRELIMINARIES. — In this experiment, the colours are to be presented serially, in spectral order (or reversed spectral order). They must therefore be numbered, 1 to 27, in that order: tint coming after tint, and shade after shade, as in the curves of the preceding Experiment.

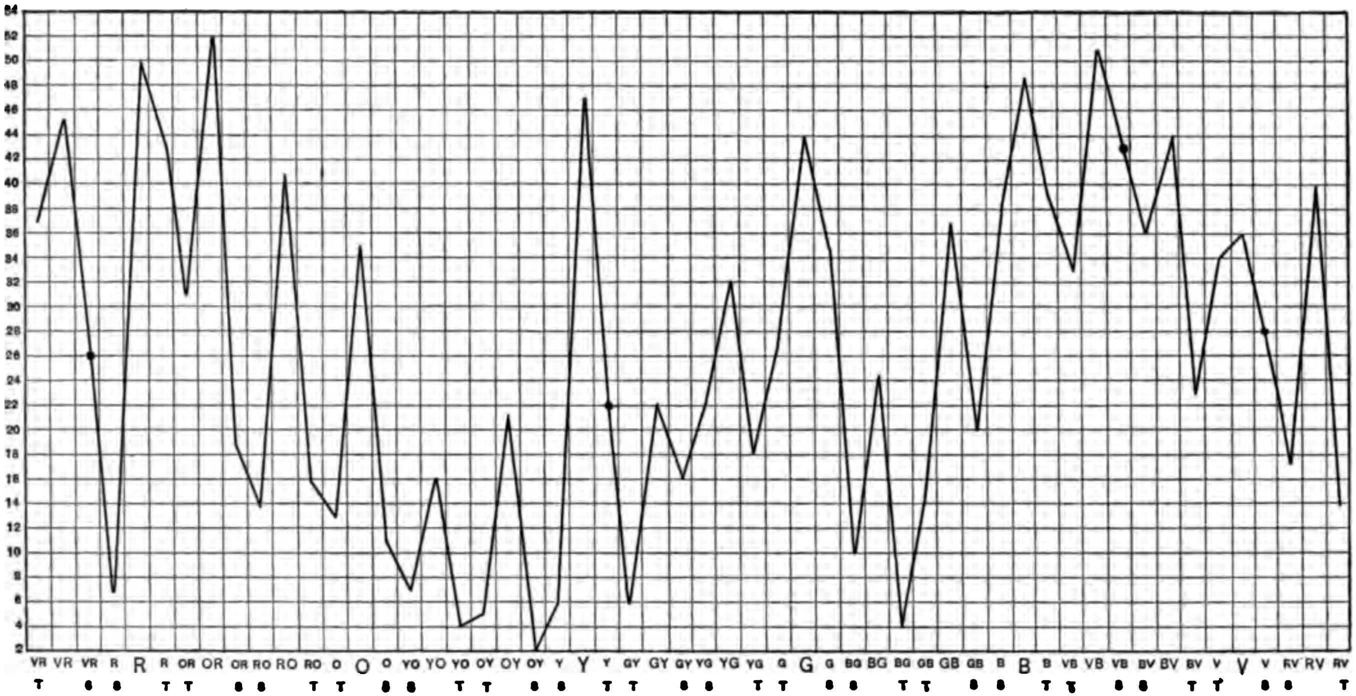


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FIG. 19.

O is now called upon to learn and standardise a scale of arbitrary affective values. Seven steps can readily be held in mind:

1. Very pleasant.
2. Moderately pleasant.
3. Just pleasant.
4. Indifferent.
5. Just unpleasant.
6. Moderately unpleasant.
7. Very unpleasant.

This scale must be memorised, and tested in preliminary experiments. It should be written clearly, *e.g.*, on a blackboard, in the experimental room, in order that *O* may refresh his memory of it at the beginning of each series. As soon as he thoroughly understands in what sense a colour may be called 'very pleasant,' 'moderately unpleasant,' etc., — and he must learn this by actual experience, since (as was said above, p. 151) the affective value of all colours is, absolutely taken, very small, — he will find no difficulty in applying the scale, and his judgments will be surprisingly constant from series to series and from day to day. He must not assume the æsthetic, but the psychological attitude; he must wear off the 'feeling of unaccustomedness' that arises at first, when he is set down to find 'an uninteresting colour patch' pleasant or unpleasant; and he must also wear off the feeling that in such an experiment everything is uncertain and subjective, and that the results must of necessity be valueless. Attentive work soon overcomes these difficulties of the affective judgment.

Experiment. — The grey card is set up on a table, at a convenient distance (perhaps 2 m.) from *O*. *O* sits with his eyes closed. *E* places one of the coloured squares, chosen at random from the full series, behind the window of the screen. The upper edge of the paper may be lightly pasted, or a pin may project through the card and the paper be hung upon its point. The illumination must be uniform and permanent. At the word of command, *O* opens his eyes, and looks steadily at the colour. After 2 sec. the colour is covered or removed, and *O* enters his judgment (1, or 2, or 3, etc.) on a paper at his side. He may write out any introspective observations that occur to him; but these should not be encouraged, unless they refer to

sources of error. He then closes his eyes. After the lapse of 10 sec., *E*, who has placed the colour next in spectral order (to right or left) behind the screen, gives a second signal, and the experiment is repeated. In this way, *E* works during a single sitting straight through his colour series to the end (or beginning) of the spectrum, and then round again from the beginning (or end) of the spectrum to his starting-point. At the end of the experiment, he has materials for platting a curve: the abscissæ are the colours, in spectral order; the zero-ordinate is the affective value 4, indifference; and the positive and negative ordinates are the values 1–3 and 5–7 respectively.

The experiment should, however, be repeated three or four times; each time from a different starting-point (absolute, as red, blue, etc.; and relative, as tint or shade), and each time in a different spectral direction (to right or to left). The curve values must then be averaged from the combined results. If there is fluctuation of judgment as regards any particular colour, the mean of the different figures must be taken and entered in the curve. It will be found, as was said just now, that (when once the norm has been established) the judgments are surprisingly constant.

Points to notice are the following. (1) If the method is to be valid, *O* must keep in mind the serial nature of the impressions. There is no difficulty about this: the coming colours are expected in a definite order, and each little group of tints-colour-shades is apprehended as a link in a total chain of colour. Still, the point must be noticed. Any tendency on the part of *O* to judge of the colours as isolated, separate, independent stimuli would be disastrous for the method. (2) The affective curve in this case is no more absolute than are the curves of the preceding Experiment. There is no guarantee, *e.g.*, that 7 is as far below 4 as 1 is above it; the size of the steps above and below the abscissa may be, absolutely, very different. And again: there is no guarantee that the figures as applied to colours mean the same thing as they would if applied, *e.g.*, to smells. Indeed, the contrary of this is pretty obvious: a 'very pleasant' smell is a great deal more pleasant than a 'very pleas-

ant' colour. Within its limits of relativity, however, the curve is accurate enough. (3) Careful watch must be kept for possible errors residing in memory of past judgments of similar impressions, fatigue due to the tedium of a long series progressing in a known direction, influence upon present judgment of the judgments last passed, etc. The first of these will hardly enter into the results of a conscientious observer; the second can be guarded against by observation of the attitude of the observers to the work; the third can be avoided both by explicit directions to *O*, and by instructing him to fold his record paper down after each writing, so that he does not see how many '1' or '2,' etc., he has written in a given hour.

LITERATURE. — D. R. Major, On the Affective Tone of Simple Sense Impressions. Amer. Journ. of Psych., vii., 1895, 57 ff.

J. Cohn, Gefühlston und Sättigung der Farben. Philos. Stud., xv., 1899, 279 ff.

EXPERIMENT XXII

§ 39. **The Affective Qualities: Involuntary Movement.** *Cautions not noted in the Text.* — Very great care must be taken, in this experiment, to avoid any suggestion to the students as to what result is to be expected. We know, from various 'spiritualistic' tests and reports, how extremely suggestible the planchette is: and the automatograph is merely the planchette, renamed for scientific uses. If a pair of students chance to know, from reading or lectures, what the correlation between affective quality and involuntary movement is, they should be cautioned (*a*) to tell no one else in the laboratory what they know, and (*b*) to keep themselves, as far as possible, without prejudice. There will, in all probability, be several students who do *not* know the correlation: their results may be used as a check upon the rest, and displayed to the whole class at the conclusion of the experiment. Those who do know will, probably, get 'too good' tracings at first; then they will, by counter-suggestion, get no correlation at all; but finally, if they are serious and painstaking, they will be able to show records of precisely the same character as those obtained from ignorant subjects. — All this shows the necessity (mentioned in the text) of preserving

every record, together with its introspective label, and entering an account of the complete experiment through all its stages in the laboratory note-book. If the correlation is not realised, there is always some definite reason for failure; and the observation of 'bad' records, alongside of the introspective account of their conditions, may be as instructive as that of a page of 'correct' results.

An obvious difficulty in the way of this experiment is that *E* is observing the correlation, which it is the object of the experiment to establish, from the moment that he begins to subject *O* to pleasant or unpleasant stimuli. It can partly be overcome by instructing *E* not to compare the tracings with *O*'s introspections until he himself has served as *O*. He is simply to continue the experiment, with the scent-series at his command, until *O* says: "I have had my six good and six bad smells." The affective value of the stimuli varies so largely with different *O*'s, that *E*'s conjectures and expectations will probably be much at fault, and he will enter upon his own introspective task with an open mind. If, however, the experiment chance to proceed so smoothly that the correlation comes out from the first, *E* must be treated in the same way as the other students who know beforehand what their results should be.

The experimenting room should be as free from foreign odours and from draughts of air as possible.

MATERIALS. — There is another form of automatograph on the market: a sheet of glass, sliding on three steel balls upon another sheet, on which in turn the paper is placed. The upper glass carries the subject's arm and hand, and is furnished in front with a projecting piece in which the stylus is set. This apparatus is, however, much more expensive, while it is mechanically less satisfactory, than that described in the text.

Tin dishes filled with odorous material are recommended because they offer a fairly wide surface to the nostrils, and because they can easily be scalded, for cleaning, without fear of breakage. Glass tubes, widening above, the cup-shaped end filled with cotton-wool soaked in the required liquid, may be substituted for them in certain cases: but the results are less satisfactory.

The following scents may be recommended:

(*a*) Pleasant.	(*b*) Unpleasant.
Crab-apple blossom.	Carbon disulphide.
White rose.	Rancid cheese.
Oil of orange.	Wood alcohol.
Jockey Club.	Asafœtida.
Heliotrope.	Castor oil.
Oil of cinnamon.	Cod liver oil.

The above will be found pleasant and unpleasant, as described, by most observers. Stimuli that vary in their affective value from one *O* to another, but usually produce marked effect, are: spirits of camphor, oil of cloves, oil of peppermint, oil of anise, tar, kerosene, citronella oil, oil of cajeput. Others will doubtless suggest themselves in the course of the experiments, if they are required. No stimulus should be employed which sets up marked pricking, burning, choking, etc., sensations in addition to the smell quality proper.

EXPERIMENT. — Care must be taken that *O* does not change his manner of breathing when the stimulus is applied. There is a temptation to 'take long breaths,' which should be resisted. The normal tremor with this apparatus is a pulsating movement, — travelling laterally (from left to right, or from right to left, indifferently) over a space of 1 to 3 mm. The line will often return upon itself, so that its waves are obliterated, and only a small irregular white blotch is left on the paper. The objective tests of a true normal are (*a*) small excursion of the point, and (*b*) inconstancy of direction. To these the subjective test of introspection must be added.

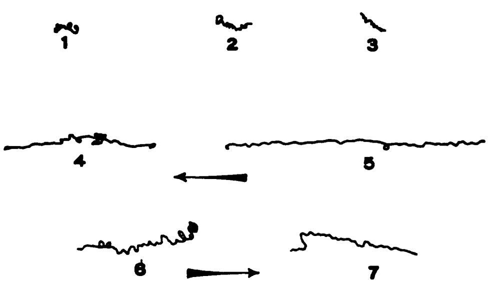


FIG. 20. — Records of involuntary movement. 1, 2, 3, normal tremors; 4, 5, 'unpleasant' tracings; 6, 7, 'pleasant' tracings. The records have been somewhat simplified in transcription from the smoked paper, but their dimensions are accurately shown.

With distinctly pleasant stimulation, the point travels laterally outward (*i.e.*, in the experiment described, from left to right); the arm is extended. The pulsations of breathing are apparent. There is no return of the line upon itself, and (for the most part) no arrest of movement at any point. The tracing may reach the length of 2 cm., while the movement is still entirely involuntary, *i.e.*, unknown to *O*.

With distinctly unpleasant stimulation, the point travels inward; the arm is flexed. The line is usually flatter than in the

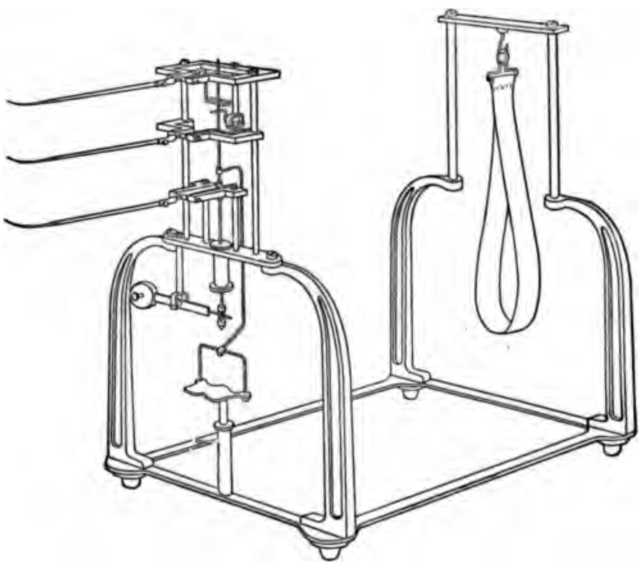


FIG. 21.

preceding case; the breathing waves are less apparent. In the case of an odour so unpleasant as that of rancid cheese, the tracing may be as long as 4 cm., and still give no evidence of itself to introspection.

QUESTIONS. (1) Yes. Pleasantness is correlated with movements of extension; unpleasantness with movements of flexion.

(2) Pleasant things are 'naturally' the things that we reach out after, open our arms to; unpleasant things are those that we shrink from,—things that make us 'draw ourselves in' when we meet them. Since, in the main, pleasant things are biologi-

cally good for us, and unpleasant things bad, the biological sanction of these movements is evident.

(3) Probably, the unpleasants are the stronger. It is usually easier to find a scent that *O* 'loathes' than to find one that he 'loves.'

(4) The idea might 'suggest' an imitative movement to the planchette. If *O* were thinking, *e.g.*, of a horse, or person, or name, the stylus might tend to trace the outline of the horse or person, or the letters of the name.

INSTRUMENTS. — The automatograph referred to above, p. 159, is that of J. Jastrow (Amer. Journ. of Psych., iv., 1892, 398; v., 1892, 223): sold by the Chicago Lab. Supply Co. for $6.00. A simpler form is sold for $1.00. Fig. 21 illustrates the tridimensional analyser of R. Sommer (Zeits. f. Psych., xvi., 1898, 275): sold by Schmidt, Mk. 85. For E. B. Delabarre's finger-movement recorder, see L'Année psych., i., 1895, 532 (Verdin, Fr. 120).

LITERATURE. — H. Münsterberg, Beiträge zur experiment. Psychologie, iv., 1892, 216; G. van N. Dearborn, Psych. Rev. Mon. Suppl. 9, 1899, 33 ff. (*cf.* Psych. Rev., iv., 1897, 453).

EXPERIMENT XXIII

§ 40. **The Affective Qualities: Dynamometry.** *Cautions not noted in the Text.* — The difficulty arises here that arose in Exp. XXII.; *E*, even if he were ignorant of the correlation before the experiment began, will learn it in the course of his observation of *O*'s pulls. Fortunately, the 'suggestion' is not serious. The movement in the present instance is voluntary; the pull is to be consciously maximal. Any 'suggested' letting-up of pull, on unpleasant stimulation, and any similar increase in pull, on pleasant stimulation, will reveal themselves in introspection as artificial. One cannot honestly deceive oneself as to whether one is pulling one's hardest or not. *E* will therefore be able, by attending *to* the pointer and *from* the suggestion, to give clean results.

Some *O*'s declare from the outset that they cannot make a maximal effort without pain. Such statements can always be traced, on examination, to an awkward position of the wrist or an insufficient padding of the hook.

Since the experiment requires a continuous attention for 1 to 1.5 min., and *O* is, by hypothesis, not a very highly trained observer, there will almost certainly be a number of 'bad' records due to distraction (see below, answer to Question 5). It is well to bear this in mind, — both because the fact of inattention may help to explain results otherwise inexplicable, and because it prevents the Instructor from making too severe demands upon *O*.

Plenty of time for rest must be allowed between experiment and experiment.

MATERIALS. — By a very simple arrangement of cord (tied to the pointer bar), counter-weight and pulleys, a writing-point can be attached to the dynamometer, and the curves traced continuously (not taken merely at 5 sec. intervals) on the smoked paper of the kymograph. The details of the effect of pleasant and unpleasant stimulation then come out more clearly. For all practical purposes, however, the arrangement described in the text is sufficient.

Care must be taken not to overfill the bulbs of the syringes. A too large quantity of the stimulus solution fails of its effect; the vomiting reflex is set up, and *O* spits it all out. — Many other devices for the application of stimulus have been tried. *O*'s head is bent down, with the effort of pulling, so that his mouth is hard to reach. The syringe, carefully used, has been found preferable to burettes, bent spoons, flexible tube and bottle, etc.

The taste solutions cannot be prescribed, as there are great differences of individual like and dislike. The following will, however, probably be found useful: castor oil, cod liver oil, quinine (.002 to 1% solution of the hydrochlorate), strong coffee, 33% of 95% alcohol, essence of wintergreen (5% solution), essence of peppermint (5%), essence of anise (5%), syrup of orange (15%), tar (wine of tar, 5%), lime juice (10%), syrup of cherry (15%), maple syrup.

EXPERIMENT. — (1) The normal curve in this Experiment approximates very closely to a straight line; *i.e.*, the strength of pull decreases in direct proportion to the time of pull. After a certain time has elapsed (60 to 90 sec., as mentioned in the

text), the curve runs at a low level parallel with the abscissæ; it is useless to continue the experiment to this point, as *O* merely becomes extremely fatigued, — and thus badly disposed for the following experiments. The simple character of the normal curve makes it an easy matter to determine the affective correlations required.

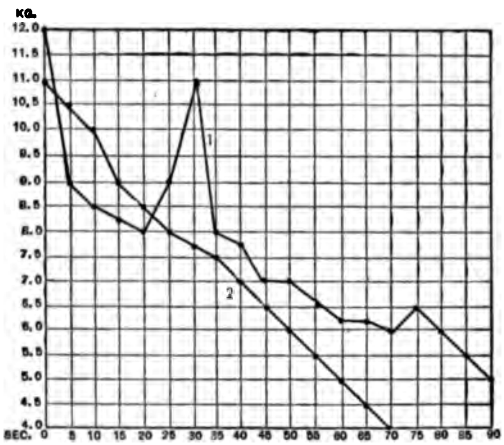


FIG. 22. — Two 'normal' records from the same *O*: 1 the first, 2 the third taken. Shows the regularising effect of practice.

(2) Since we do not ask *O* to plug his nostrils, we are stimulating the organs both of taste and smell; the first expiration, after the injection of the liquid into the mouth, will set up a smell sensation. This double effect increases the affective value of the stimuli employed. But it means, of course, that care must be taken to prevent any premature recognition of the stimulus by *O*. The syringes must be filled in a different room from that in which the experiments are performed, and their tubes kept closed until the moment for use arrives.

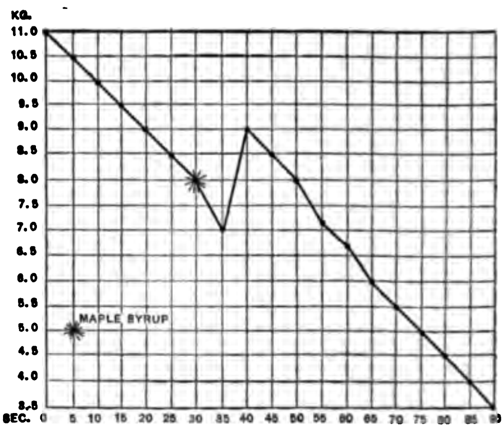


FIG. 23. — Typical curve for high degree of pleasantness.

The reason that the stimulus is applied in the form of a taste,

rather than in that of a smell, is that *O* is more likely to be distracted by the arrangements for giving the latter. We must remember that *O* is concentratedly attentive to the visual impression of the pointer. When we apply our stimulus, we must try not to divert his attention from this impression. Our object is to bring it about that, whereas he was before merely 'attentive to the pointer,' he is now 'attentive influenced-by-castor-oil [or whatever the stimulus may be] to the pointer.' We have no desire to make him inattentive to the pointer, and attentive to the castor oil. But the preparations for olfactory stimulation prove, in practice, to have a much greater distracting effect than does *E*'s putting out of the hand to squeeze the bulb of the taste-syringe.

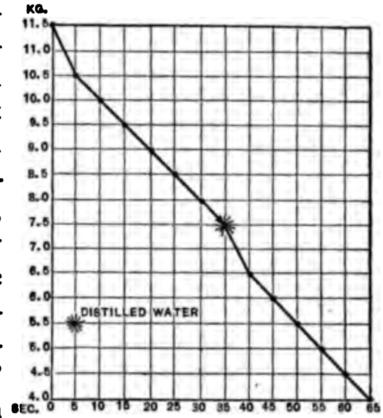


FIG 24.—Typical curve of 'blank' experiment.

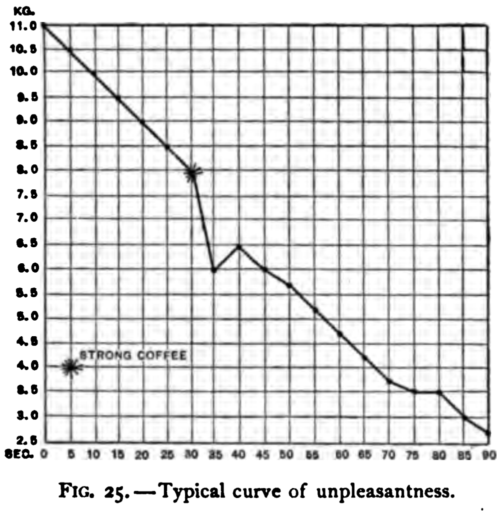


FIG. 25.—Typical curve of unpleasantness.

The correlation, as shown both by these rough 5-second curves and by the continuous tracings of the kymograph, is as follows. When a *very pleasant* stimulus is applied, the curve drops a little, and then quickly rises again, to a point above the level of the normal. This higher level, relatively to the normal, is maintained till the end of the experiment. When a *very unpleasant* stimulus is applied, the curve takes a decided drop ; then recovers a little ; but

remains for a long time (if not till the end of the experiment) relatively lower than the normal.

Distilled water, and other indifferent stimuli, leave the normal curve unaffected, save for a slight drop immediately following the injection.

QUESTIONS. — (1) Yes. The pleasant stimulation makes us stronger, the unpleasant makes us weaker.

The question may arise, why the 'pleasant' curve does not rise at once, on the introduction of the stimulus, instead of dropping and then rising. The answer seems to be, that the drop corresponds to a momentary distraction of attention from the work in hand, — a dividing or confusing of consciousness, due to the interruption of the hitherto undisturbed effort. The pointer does not hold its place at the focus of consciousness, and consequently does not hold its place on the scale. As soon as the confusion is over, and consciousness settles down to renewed concentrated attention, the effect of the stimulus becomes apparent. As was noted above, the drop will be found in the 'blank' curves as well as in the pleasant and unpleasant records. — *Cf.* Lehmann, Hauptgesetze, 95.

(2) Things that are biologically good for us (the pleasant things) would 'naturally' make us stronger; and things that are biologically bad for us would make us weaker.

(3) The pleasant.

(4) Yes: the strengthening of pull is more clearly marked and longer continued, in most cases, than the weakening.

(5) The distraction, mentioned above. It is not difficult, in Exp. XXII., to see what is meant by exchanging an 'odourless dreamy reverie' for an 'apple-blossom dreamy reverie'; not difficult to let one's thoughts float off in odour, while previously they had floated away uncoloured by smell sensation. On the other hand, it is not easy here for *O* to grasp what is required of him, and not easy for him, at any rate in the first few trials, to act out what he has understood. He is far more likely to attend to the taste impression than to give a taste attention (in place of the previous tasteless attention) to the pointer. Close introspection enables him to overcome the difficulty.

ADDITIONAL QUESTION. — We have spoken in the text of

'the attention being concentrated upon the pointer,' of 'the pointer being at the focus of consciousness,' etc. Such expressions are, of course, very imperfect representations of the reality. The attentive consciousness of the experiment consists (1) of the visual perception of the pointer and the tactual perception of the 'effort' exerted, (2) of various peripheral cues, which taken all together spell 'effort,'—tightening of the scalp, setting of the jaw, frowning, clenching of the unemployed hand, settling of the head down upon the shoulders, etc., and (3) of ideas, clustering round and reinforcing the two primary perceptions, and differing in source and composition from individual to individual.

If *O* is sufficiently advanced, he may be required to analyse, by introspection, his own attentive consciousnesses during the experiment. If the task appears too difficult, the Instructor should indicate to him, in general outline, the processes of which these consciousnesses are composed.

§ 41. **Alternative Experiment.**—This experiment may be performed, not quite so easily, as follows.

MATERIALS.—Hand dynamometer. [This consists of an oval steel frame, about 12.5 cm. long, and 6 cm. broad at its widest part, carrying within it a gear system whereby the amount of squeeze in kg. is indicated by a pointer upon a scale.]

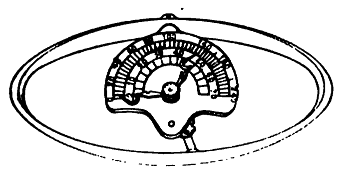


FIG. 26.—Hand dynamometer (Zimmermann, Mk. 27.50; Collin, Fr. 35; Verdin, Fr. 25). The instrument has two kg. scales, an inner scale for squeeze, and an outer scale for pull.—A dynamograph is sold by Verdin for Fr. 130.

Set of pleasant and unpleasant odours.

Small shallow tin dishes.

PRELIMINARIES. — The dynamometer must be carefully padded with cotton wool and soft cloth, so that the squeezing is not painful. *O* must practise the squeeze, taking care that (*a*) the position of arm and hand, and (*b*) the time of squeeze remain constant. Thus, the squeezes may always be taken with the arm held down straight by the side of the chair; and the squeezing may be done in time with the beats of a metronome. When *O* has attained to a fair

degree of regularity in these respects, and when his maximal squeezes are fairly constant, the experiment may begin.

Difficulties will almost certainly arise in regard to the stimuli. The odours required for the experiment must be very distinctly pleasant and unpleasant, if their affective correlation is to come out clearly. It is well (*a*) to find out from *O* beforehand what smells he likes and what he dislikes, and to choose the stimuli accordingly; and (*b*) to record the affective value of each stimulus actually employed in terms of an arbitrary scale. Thus:

1 might mean 'indifferent,'
2 " " 'moderately pleasant' or 'unpleasant,' and
3 " " 'distinctly pleasant' or 'unpleasant.'

This scale is explained to *O* before the experiment begins.

EXPERIMENT. — (1) We first find our normal. *O* sits at the table with closed eyes. The dynamometer is put into his hand. At the word "Now!" he gives a maximal squeeze. *O* then hands the instrument to *E*, who records the kg. reading of the scale.

(2) *O* sits as before. *E* puts the dynamometer into his hand. At the word "Ready!" a dish of smell solution is held under his nostrils, and he takes three full breaths. Then, at the word "Now!" he squeezes as hard as he can. *E* again takes the reading. *O* assigns to the stimulus the value 1, 2 or 3, as the case may be.

To make the results of this experiment pure, the scents should work simply and solely for the three breaths, and for no longer. They should be kept in a different room from that in which *O* is sitting, and should be carried out of the room as soon as they have been smelled. Moreover, the room should be thoroughly aired out between test and test.

Another great source of error is fatigue. Not more than six tests should be taken in the course of an hour, and these should be so arranged that the effects of practice and fatigue are as nearly as possible equalised. It is not well that *O* and *E* should alternate, test and test about, — so that the hour's work gives 12 tests; for *O* must remain passive and steadily disposed throughout the sitting, if the results are to be valid. To pre-

vent tedium, *O* may be allowed to fill in the time with some light and simple occupation: sewing, knitting, etc., magazine or newspaper reading, easy laboratory tasks such as the cutting of coloured paper discs, have proved suitable. Conversation is apt to be exciting, and to spoil the results.

The following are typical results (*O* a woman). — Right hand squeezes. Unit 1 kg.

Normal.	Pleasant.	Unpleasant.
23.0	26.5 (crab-apple blossom)	21.0 (carbon disulphide)
24.0	27.0 (white rose)	22.0 (wood alcohol)
23.0	25.0 (oil of anise)	21.0 (stale cheese)
25.0	26.0 (spirits of camphor)	22.5 (burnt hair)

In all these results the stimuli had the affective value 3 assigned to them by *O*. The readings are to the nearest half-kg. Each set of 6 experiments was made at a single sitting.

It is clear that the 'pleasant' squeeze is always the strongest, the 'unpleasant' the weakest. If we average the results, we get:

Normal 23.7 ± .7 Pleasant 26.1 ± .6 Unpleasant 21.6 ± .8.

This interrelation remains constant from day to day, although the normal varies somewhat (even after practice) with variation in *O*'s general condition, and the relative effect of pleasant and unpleasant stimulation varies also with *O*'s disposition at the time of the experiment. The following series, from the same *O*, was taken on a day when the observer was feeling unusually well.

Normal.	Pleasant.	Unpleasant.
27.0	27.5 (oil of cinnamon)	24.5 (stale cheese)
27.0	28.5 (oil of anise)	21.5 (rubber cement)

or, averaged:

Normal 27.0 ± 0 Pleasant 28.0 ± .5 Unpleasant 23.0 ± 1.5.

Here the unpleasants had more power to reduce the muscular strength than the pleasants had to increase it.

In the results quoted, the differences are all too great to be explained by chance. But were they smaller, their unbroken constancy throughout a long series of trials would be strong evidence in favour of their validity.

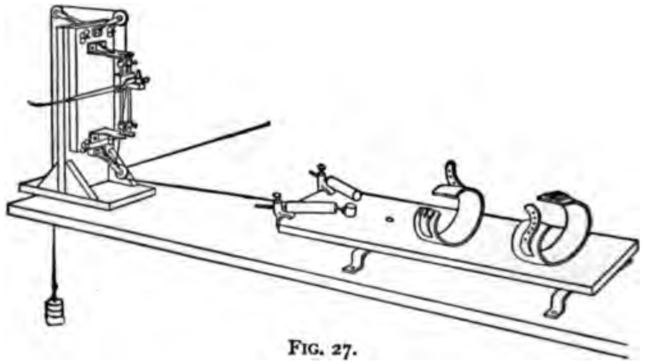


FIG. 27.

INSTRUMENTS. — Fig. 27 shows A. Mosso's ergograph and arm-rest (Willyoung, $45; Verdin, Fr. 175); Fig. 28, J. McK.

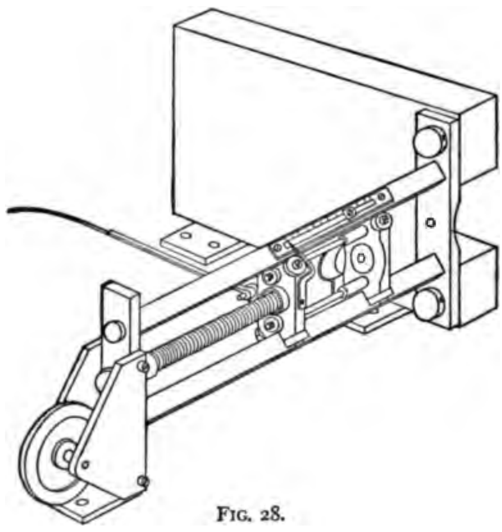


FIG. 28.

Cattell's combined spring and weight ergograph (Horstmann, Columbia Univ. Lab., $35). Another spring ergograph is figured and described by A. Binet and N. Vaschide, L'Année

psych., iv., 1898, 303. The ergograph may replace the dynamometer in this experiment. For its use, see A. Mosso, La fatigue, 1894, 53 ff.; A. Binet and V. Henri, La fatigue intellectuelle, 1895, 175 ff.; A. Binet and N. Vaschide, L'Année psych., iv., 1895, 253.

LITERATURE. — On the general correlations brought out by this and the following experiment, see Külpe, Outlines, 245 f.; A. Lehmann, Die Hauptgesetze d. menschl. Gefühlslebens, 1892, 82, 86, 89, 91, 112. *Cf.* also A. Binet and J. Courtier, L'Année psych., iii., 1897, 65 ff.

On the use of the dynamometer, see Binet and Henri, La fatigue intellectuelle, 1895, 172; A. Binet and N. Vaschide, L'Année psych., iv., 1898, 245.

EXPERIMENT XXIV

§ 42. **The Affective Qualities: the Plethysmographic Method.** *Cautions not noted in the Text.* — The difficulty of suggestion to *E* again confronts us. But both *O* and *E* should now know enough about affective work to realise that 'honesty is the best policy.' And *O*'s introspective accounts (of which more presently) can only serve to confirm *E* in his resolve to resist suggestion, and let the experiment take care of itself.

E must not look for change in the curve at the precise instant of the application of stimulus: a stimulus takes an appreciable time to act. Neither must he assume that the change will end at the precise instant of the removal of stimulus: the after-effect, as will be seen presently, is an integral part of the phenomenon under observation. On the other hand, the change must always be definite, clean-cut, restricted in time. If this is not the case, there are sources of error, physical or psychological, which have been left out of account.

MATERIALS. — The two kinds of stimulus best adapted for this experiment seem to be those of smell and hearing. Taste stimuli can hardly be administered without some shaking and jarring of the immersed arm, while for light stimuli the eyes must be opened, — and it is best on all accounts to keep them closed. Touch can scarcely be appealed to, as the immersed

arm is giving clear and massive sensations of pressure and temperature. On the other hand, agreeable and disagreeable smells are easily found and easily presented: *O* must, of course, be cautioned to take them passively, and not consciously to change his type of breathing while they are under his nostrils. Sound stimuli — chords and discords on forks or piano, musical phrases, harsh noises — are still cleaner, but less strongly affective.

However, the stimuli must be chosen to suit the subject; and any stimulus should be unhesitatingly taken which promises to bring out a well-marked reaction. It may be necessary to provide a second *E*, to give the affective stimulus, while the original *E* attends to the drum.

The kymograph is one of the corner-stones of laboratory equipment. We may therefore describe its use in some detail.

The Kymograph and Its Use. — The recording apparatus and accessories, required for this and similar experiments, are as follows.

(1) Clockwork kymograph, with two drums.
(2) Supply of kymograph paper.
(3) Stand to take revolving drum during smoking.
(4) Lamp for smoking.
(5) Stand to take spare drum.
(6) Sharp scalpel.
(7) Varnish, varnishing tray, hangers.
(8) Marey tambour and writing point.
(9) Tubing and air-cock.
(10) Time-marker.
(11) Standards, with right-angle clamps and spare arms.

(1) The *kymograph* (Gk. κῦμα, wave, and γράφειν, to write) is so called because it was first employed for the recording of curves of blood pressure. It is, however, well adapted to record any process whose course is a function of time elapsed. It consists, in essentials, of a hollow brass cylinder, the ***drum***, which is rotated at a constant rate by means of a clockwork (water motor, weight, electric motor, etc.). The rate of rotation may be varied, within fairly wide limits, by change of governor, or some similar device.

As the drum revolves, it presents a continuous writing-surface, — precisely the same surface, of course, that would be afforded if the cylinder were slit down its length and spread out flat; but in more compendious form, and under better control.

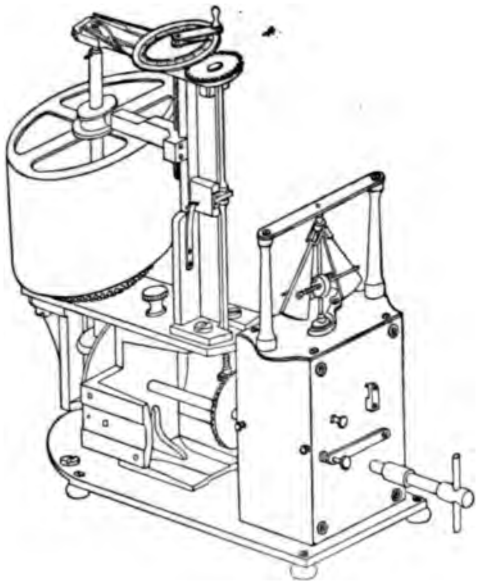


FIG. 29. — The Ludwig-Baltzar kymograph. Zimmermann, Petzold, etc.; with accessories, Mk. 900. See Langendorff, Physiol. Graphik, 1891, 19.

(2) Instrument-makers supply *kymograph paper* with their kymographs. It is a fairly tough, glazed paper, cut in strips whose width is the height of the drum and whose length is about 5 mm. more than the circumference. The overlapping edge is gummed on its unglazed side.

The cylinder is laid in the smoking stand, no. (3). The gummed edge of a strip of paper is moistened, and the paper passed under the drum, glazed side outwards; the edges are brought together above and the gummed edge pressed down smoothly and snugly over the other. The paper should fit the drum squarely, and should show no crease or other unevenness.

(3) The *smoking stand*, in its simplest form, consists of two upright strips of wood on a wooden base. The strips are cut out, in U-shape, at the top, so that the axis of the drum may rest upon them: their distance apart must be regulated by the length of this axis. The papered drum is laid on the stand, and its axis revolved by the fingers of the left hand.

(4) The right hand holds the *smoking lamp*, a small petroleum-burning lamp with broad wick. As the drum is turned, the

lamp-flame is held close up under it, and the lamp moved fairly quickly from right to left and back again, so that the smoke traces broad spirals of soot upon the white paper. The rate of turning must be learned by practice. The paper should be evenly, but not too thickly, coated with the brownish-black soot. It is, upon the whole, better to work with a mere grey film of soot than to have the drum-surface overloaded; though the right amount of smoking varies greatly with the nature of the writing-point.

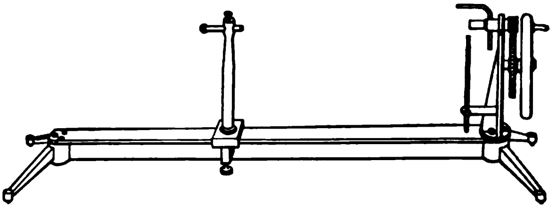


FIG. 30. — Universal smoking stand. Zimmermann, Mk. 50.

The smell of a freshly smoked drum is very intensive, and for some *O*'s very disagreeable. For this reason, if for no other, the smoking should never be done in the experimenting room. For the same reason, the drum should be allowed to cool, before it is put on the instrument. Here is one of the advantages of the spare drum: the two drums can be smoked together, and the empty one put in place as soon as ever the first is filled, without long interruption of the experiment, and without the prospect of exposing *O* to an unpleasant smell stimulus.

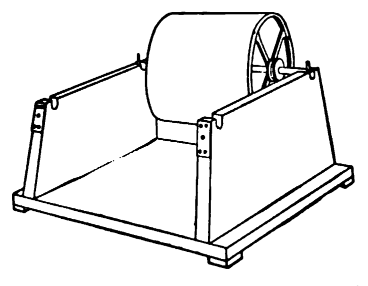


FIG. 31. — Stand for spare drum (Ludwig-Baltzar Kymograph).

(5) The *stand* for the spare drum may, again, be very simple: just two uprights, cut into U-shape above, to hold the second drum while it is waiting for use, or the first drum after it has been filled. A small deal packing-box makes a very good stand of this kind. The smoking stand might be used; but it is much more convenient to have a stand in each room.

(6) To remove the smoked paper from a drum, proceed as follows. Take the axis of the drum in your left hand, resting your thumb upon the edge of the drum and therefore upon the edge of the paper. Let the place of contact be the seam of the paper. Draw the *scalpel* sharply up, along the seam, cutting outwards so as not to injure the drum. When the cut is made, raise your thumb partially, so that the side of the paper that is farther from you slips down from the drum. While this is happening, secure the other end of the hither edge by a clip, or by the fingers and thumb of your right hand. Lay the drum on the stand, and remove the paper entirely. Lay it out flat on a table.

Some students prefer to remove the paper while the drum is on the stand. Two courses are then open. (1) Turn the drum seam upwards. Pass the fingers of your left hand into the drum, and hold the paper at the seam with the thumb. Cut the seam. Pick up the hither edge of the paper with a clip, and draw the paper all sharply upwards, while the left hand gives the drum a quick turn out. (2) Place the drum as before. Cut the seam, but leave a narrow strip uncut at the right-hand end. Turn the drum down, through some 170°, and take the nearer edge of the paper in a clip. Pull sharply on the clip, towards yourself, at the same time that your left hand gives the drum a quick turn in. The strip tears away evenly, and the paper comes in over the drum.

(7) After the record has been numbered, dated, etc., it must be varnished for permanent keeping. The varnishing outfit consists of (*a*) a flat *tray* or shallow dish (a baking dish of agate ware makes a good tray), at one end of which a hole is punched. A cork is fitted into the hole, and a short piece of glass tubing passed through the cork. The glass tube is connected (*b*) by a long rubber tube to the *varnish bottle*. This is a large and wide-mouthed bottle, corked or stoppered, containing (*c*) the *varnish* which is to be floated over the record. The varnish may be made up on various recipes: the most satisfactory is, perhaps, a solution of 10 parts of white shellac in 100 parts of 90% alcohol. The mode of varnishing is as follows. The bottle is raised to a shelf above the varnishing tray. The bottom of the tray is thus flooded with varnish. The record is taken up from the table by two clips or forceps, one in each hand, and drawn slowly and evenly through the solution, face upwards. All parts of the

smoked surface must be covered by the varnish. One of the clips is then released, and the record drawn out and hung up to drain.

The ordinary hanging clips sold by picture dealers, having a spring clip below and a curved hook above, make (*d*) useful *clips* for holding and suspending the records. Behind the varnishing tray stands an upright wooden frame, carrying (*e*) projecting arms or *hangers*. The hooks are slipped over these arms, and the records drip into the tray below. When the record has been hung up, the bottle is lowered to a shelf below the tray, and the varnish runs back. The cork or stopper must be removed while the bottle is emptying and filling, but should be carefully replaced when the work is concluded.

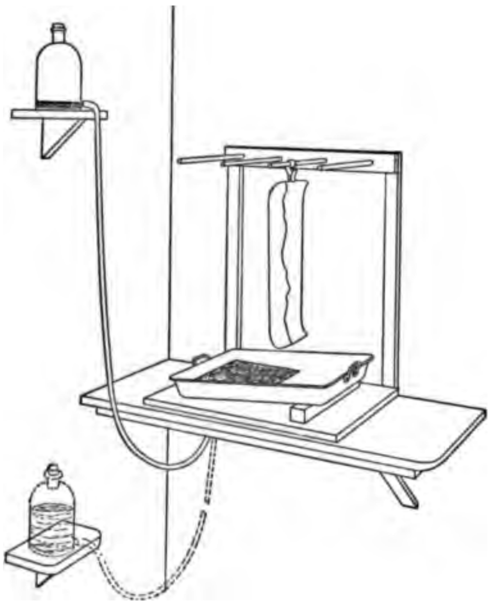


FIG. 32.— Varnishing tray and drying rack. $5.

As soon as the record is dry, it should be trimmed, and either pasted in the note-book, with its accompanying introspective record, or else laid without folding in a portfolio. In the latter case it must be conspicuously numbered, and a corresponding number placed over the note-book introspection. Curves easily 'get mixed'; and nothing is more aggravating than to possess a good record which cannot be certainly identified.

(8) The Marey *tambour* is, in principle, a small metal funnel, the mouth of which is closed by a piece of tightly stretched india-rubber sheeting. A small and light disc of metal, cemented to the rubber, carries the writing-point. The small end of the

funnel is connected by rubber tubing to the plethysmograph, pneumograph, etc. The writing-lever is so hung that an increased pressure of air in the rubber tube means a rise of the writing-point, while a drop in air-pressure means a fall of the point. The point thus rises and falls with expansion and contraction of the arm, inhalation and expiration, etc. The *writing-*

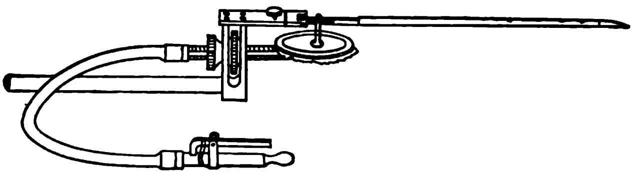


FIG. 33. — Marey tambour, writing-lever and air-cock. See Langendorff, 60; and *cf.* A. Binet, L'Année psych., ii., 1896, 776.

point consists of a curved point of parchment, steel spring, aluminium, etc., attached to a light lever. This may be of bamboo, straw, reed, etc.

Fine rubber sheeting, and rubber cement, must be kept on hand: the drum-head of a tambour is always liable to chafe or crack.

(9) A serviceable *rubber tubing* for air transmission is the sort described as 'heavy black seamless, of pure unvulcanised gum' in the catalogues of chemical supplies. The tubing should be thick-walled, and of as wide a lumen as accords with tight fitting over the metal tubules. The exact length of the pieces employed is immaterial, though there are obvious reasons for keeping them as short as possible. If the laboratory has only a small supply, and this is much in demand, it will be well to cut two-thirds of the stock into 50 cm. pieces, and the remaining third into metre pieces. Before beginning an experiment, see that there are no kinks in the system, and no doublings-under at the junction of metal and rubber; use ligatures of thread, or ease the junction by vaseline, where necessary.

One of the problems of the laboratory is to keep rubber tubing, and rubber materials generally, from stiffening and cracking. There seems to be no panacea; but the following rules are worth observing.

a. Buy rubber of good quality and of high flexibility.

b. Keep it, when out of use, in hermetically sealed jars (museum jars, or self-sealing preserve jars), or in tightly closing drawers.

c. Keep it in the dark: swathe the jars in black wrappings, or put them into dark closets.

d. Keep it plentifully sprinkled with powdered soapstone. Shake this off before using.

e. Do not be tempted to use 'experimental' rubber tubing for other laboratory purposes (*e.g.*, gas conduction): keep it strictly for its proper purpose.

f. As far as possible, avoid extremes of temperature.

For gas and water conduction, the varnish bottle, etc., rubber tubing of the sort described as 'white vulcanised' will answer every purpose.

The *air-cock* consists, first of all, of a piece of metal tubing, some 8 cm. in length, bevelled off at each end for easy insertion into the rubber tubing. The wall of the tube is pierced, near one end, by a pin-hole. Over the pin-hole lies the head of a small hammer-shaped lever, pivoted to the outside of the tube, its long axis parallel with that of the tube itself. The hammer-head is swathed in very fine rubber sheeting, and a delicate but strong india-rubber band, passed over hammer-shank and tube, holds the head tightly down in place. The efficiency of this band must be carefully tested before every experiment.

When the air-cock is left undisturbed, therefore, there is no break in the tube-system. When the extremity of the hammer-shank is depressed, the pin-hole opens communication between the air within the system and the air of the room, and normal pressure within the system is secured.

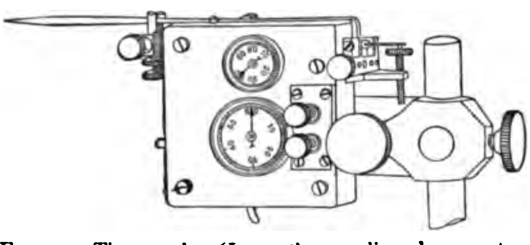


FIG. 34.—Time-marker (Jacquet's recording chronometer; marks seconds and fifths of seconds). Verdin, Fr. 170. For other instruments, see Langendorff, Physiol. Graphik, 1891, 121 ff.

(10) The *time-marker* furnishes the abscissæ of the curve of volume, breathing, etc. The time line may be obtained in various ways: from metronome, tuning-fork, interrupter-clock, etc. The simplest and most direct time-

marker, for such experiments as are here in question, is the Jacquet chronometer. This is a watch, housed in a square case, supplied with a light metal lever, whose point jerks up once in every second or once in every fifth of a second, as required. The curved point of the lever can be adjusted to write upon the drum immediately below the writing-point of the tambour.

(11) For kymographic work, a good set of *standards*, tripod bases, arms and right-angle clamps is essential. In the plethysmographic experiment we need: (*a*) a standard with clamp and arm, over which the rubber tubing that leads from jar to tambour may rest; (*b*) a standard with clamp, to take the tambour itself; and (*c*) a standard with clamp and arm to take the time-marker. Tambour and marker may be put upon a single standard, but adjustment is easier if the two are kept separate.

(12) *Adjustments.*—Care must be taken, in laying the paper over the drum, or in setting the drum upon the instrument, that the direction of writing be *from* the double thickness of paper *to* the seam, and not *vice versa.* If this rule is not followed, the writing-point will hitch over the seam, when the drum comes round to it: the point may be deranged, or a critical portion of the curve spoiled. The rule is, of course, unimportant for our particular experiments, which do not extend beyond a single revolution; but it is exceedingly important when the tracing extends over several revolutions, and is a cardinal rule, which cannot be learned too early, of kymographic work at large.

It is hardly necessary to say that the writing-points go 'with the current,' *i.e.*, that the drum moves off from under them, and not in towards them. In the latter case, any the least irregularity in the grain of the paper, or what not, would bend up the flexible point, spoil the curve, and perhaps break the lever.

When the drum is set up, swing it round so that the two writing-points may begin their records as near the seam as possible. Since we are to cut the paper at the seam, we shall in this way secure the full extent of the surface for our curve. —This rule, again, does not hold for experiments which are to continue beyond a single revolution. The seam-line may here coincide with a critical point upon the curve.

If the drum is well turned, the paper smooth, and the soot

coating even and not too thick, the friction between writing-point and writing-surface is reduced to a constant minimum. Great care must be taken that the writing-point touch the surface only at one point, and that the lever move in a plane parallel to the plane of a tangent drawn through the point of contact.

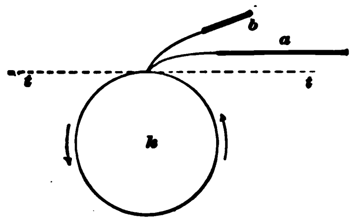


FIG. 35. — Illustrates the adjustment of the writing-point to the drum surface. *k*, kymograph; *a*, rightly adjusted, *b*, wrongly adjusted writing-point; *t*, tangential plane through the point of contact. *Cf.* Langendorff, Physiol. Graphik, 77.

The above is the merest sketch of apparatus and procedure. There are numerous types of the former, and many variants of the latter. The different instruments and modes of transmission, as well as the errors involved in tangential writing, are set forth by O. Langendorff, Physiologische Graphik, 1891. This book should be kept for reference in the laboratory library. — The above directions are not to be followed blindly; they may be modified in many points to suit the equipment and arrangement of the individual laboratory.

PRELIMINARIES. — Both *O* and *E* should understand the plan of the apparatus. Thus: the rigid metal cap is to prevent bulging of the rubber cap as the hand increases in volume. Any such 'give' at the jar-end of the system would naturally nullify the experiment. The expansion in the glass tube greatly reduces the change of water level as the hand swells and contracts; injurious pressure effects are thus avoided, while the changes are still sufficient to evoke prompt and well-marked response from the writing lever. The air-cock saves strain upon the tambour when the plethysmograph is being connected to it, and also guarantees a constant air pressure within the rubber tubes. The bent flexible tip of the writing-lever ensures the recording of the whole curve: without it the point would fly off the drum-surface at a certain height above the abscissa. — Questions of this sort must be asked and answered, until the whole scheme is clear.

Some *O*'s prefer to have the jar slightly tilted, not vertical. The instrument may then be steadied by folded cloths or towels.

If O has to move from his seat during the experiment, a roller towel can be thrown round his neck, and the jar held in it as in a sling.

The rubber sleeve will almost certainly tear away from the cap, at some point or other, before many experiments have been taken. Hence E should be provided with a tube of quick-setting rubber cement.

EXPERIMENT. — No rule can be laid down as regards a signal to O. In some cases, a signal before the clock starts is welcome, as helping towards general steadiness and passivity; in others, it is disturbing and fluttering. The whirring of the clockwork is itself a signal that the experiment has begun. Some O's are unmoved by it; others are thrown, for the first few trials, into a state of dismay, — the die is cast, and if they do not sit still now and think of nothing, all the work is lost! E must adapt his procedure to his subject: what he has to do is to convince O, somehow, that the experiment *cannot* go wrong if it is left to run its own course.

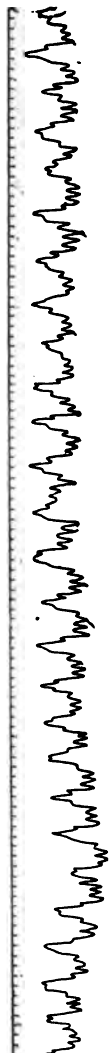


FIG. 36. – A 'normal' curve taken from a nervous O. Towards the end of the experiment, O turned his eyes to the drum, and was pleased to see a clearly marked and seemingly regular record. The pleasure shows itself in the slightly increased height of the total curve. Time unit, 1 sec.; record × ½.

It is probably true of all subjects, even the most conscientious and experienced, that they feel a certain timidity and reserve when called upon to give an introspective account of the experiment. The kymograph curve seems to be so remote and out of reach that one despairs of ever matching its impassive facts by one's scrappy and hesitating sentences. Hence the exactness of this match — the precise parallel, breath for breath, of objective and subjective repose and of objective and subjective disturbance — comes as an almost startling revelation. When once O has realised that the curve obeys his interpretation, — that it honestly reflects the turn of his head in his collar, the slight shift of his body in the chair, the unpleasant memory that forced

itself upon him, his pleased interest in the after-image of the window; while, on the other hand, it betrays everything that he has been tempted, half-consciously, to conceal,—the play of attention upon the course of breathing, his half-voluntary wish that the curve may be a good one, and his half-intention to try to make it good; he settles down resignedly into the required passive attitude, and lets the experiment go on as a matter of course. Some *O*'s see the truth at once, others require a little time: and *E* must shape his conduct accordingly.

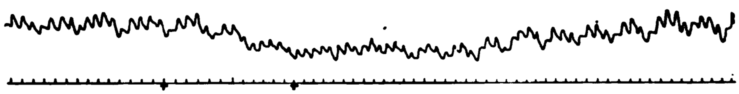


FIG. 37.—A typical curve of unpleasantness. The crosses indicate the times of application and removal of stimulus. Time unit, 1 sec.; record × ½.

It is said in the text (p. 106) that after *O* and *E* have changed places for the second time *O* is not to be told which of the two possible experiments will be taken first. It is evident that the affective experiment must be taken first, or *O* will be looking forward to it with certainty after the recording of the second normal. In all probability, however, this reflection will not occur to *O* at the time; he will regard the alternative of the text as a real alternative. If he does not, he must be told that any one of the three possible experiments—another normal, another unpleasant, or a pleasant—may be taken.

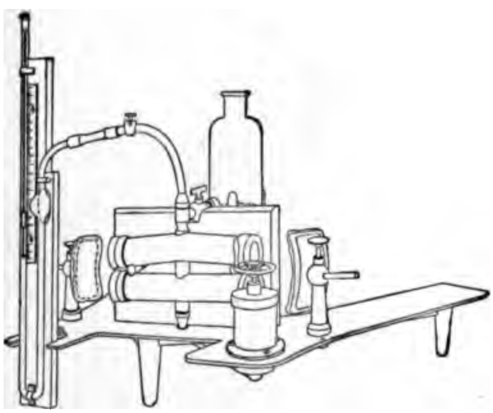


FIG. 38.—Mosso's sphygmomanometer. For a description, see Binet and Henri, La fatigue intellectuelle, 1898, 103; A. Binet and N. Vaschide, L'Année psych., iii., 1897, 129.

QUESTIONS.—(1) Yes. If a pleasant stimulus be given, the curve drops during the application, but then rises at once to a higher level and

maintains this level for some time. If the stimulus be unpleasant, the curve drops during application, and then drops still

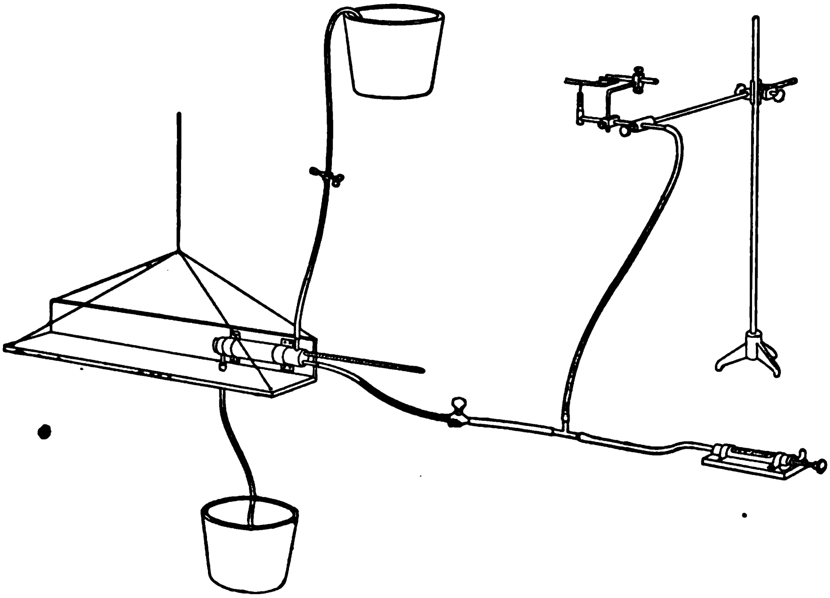


FIG. 39. — The finger plethysmograph of Lombard and Pillsbury. *Cf.* Langendorff, 68.

farther, coming back slowly to its normal height. — *Cf.* the curves of Exp. XXIII., *supra.*

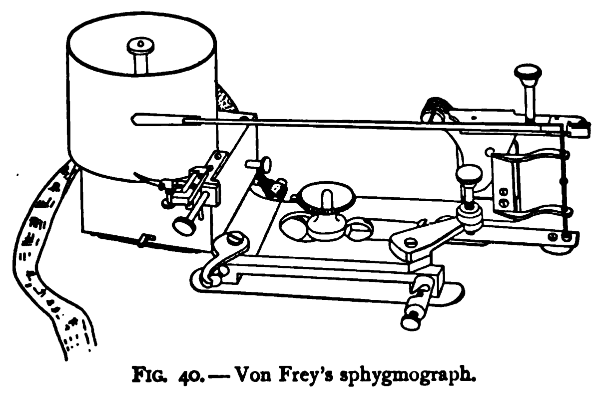


FIG. 40. — Von Frey's sphygmograph.

(2) The expansion during pleasure may be compared to the involuntary reaching-out of Exp. XXII., and the contraction during unpleasantness to the shrinking-back of the same Exp.

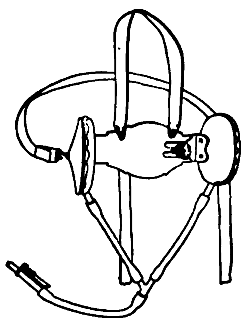


FIG. 41. — The Verdin pneumograph.

(3) In all probability there will be irregularities in the curve due to such objective and subjective conditions as were enumerated just now (pp. 181 ff.). In all cases, the curve tells an absolutely truthful story. *O* may, indeed, actually be reminded by it of some interruption which he had honestly overlooked or forgotten.

(4) Experiments should be tried upon the pulse-line, for itself, and the respiration-line, for itself. The volume curves cannot fail to suggest that these lines would vary with variation of the affective consciousness.

INSTRUMENTS. — Fig. 38 shows A. Mosso's sphygmomanometer (Verdin, Fr. 190). See F. Kiesow, Philos. Studien, xi., 1895, 41. Fig. 39 shows the finger plethysmograph of W. P. Lombard and W. B. Pillsbury, with connections (Amer. Journ. of Physiol., iii., 1899, 186). The finger tube is screwed to a swinging arm-board. Warm water, the temperature of which is regulated by a thermometer, courses through the mantle of the

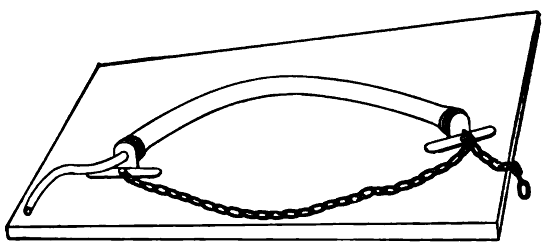


FIG. 42. — The Sumner pneumograph.

tube. The finger tube is connected, first to a 3-way cock, open to the air; and thence to a T-piece, from the one limb of which a rubber tube runs to the adjusting piston-syringe, while from

the other a similar tube passes to the piston-recorder and writing devices. — Michigan Apparatus Co., $16.

Fig. 40 is the sphygmograph of M. von Frey (Zimmermann, Mk. 200). The instrument can also be arranged for air transmission. Fig. 41 is the Verdin pneumograph (Fr. 50), and Fig. 42 the Sumner pneumograph ($2.50).

LITERATURE. — On the use of the plethysmograph, see Binet and Henri, La fatigue intellectuelle, 1898, 61 ff. (the authors figure the instruments of Fick, Mosso, Franck, and Hallion and Comte); and Langendorff, Physiol. Graphik, 1891, 235 ff. For the sphygmograph, see Langendorff, 222 ff.; M. von Frey, Die Untersuchung des Pulses und ihre Ergebnisse in gesunden und kranken Zuständen: Berlin, 1892. For the pneumograph, see Langendorff, 252; Binet and Henri, 147 ff.

The literature on this and the preceding Experiment has been brought together by J. R. Angell and H. B. Thompson, Psych. Rev., vi., 1899, 32 (Univ. of Chicago Contrib. to Philos., ii., 2, 32). To these references add Wundt, Philos. Studien, xv., 1899, 149; Völkerpsych., i., 1, 1900, 40 ff.; W. P. Lombard and W. B. Pillsbury, Amer. Journ. of Physiol., iii., 1899, 186, 201; A. Lehmann, Die körperlichen Aeusserungen psychischer Zustände, i. Plethysmographische Untersuchungen. Leipzig, 1899.

CHAPTER VIII

Attention and Action

EXPERIMENT XXV

§ 43. **Attention.** — The problem of attention is essentially a modern problem. This does not mean that the facts of attention were unobserved, and theories of attention lacking, until modern times: Braunschweiger asserts (Die Lehre von der Aufmerksamkeit in der Psychologie des 18. Jahrhunderts, Leipzig, 1899) that "it would be hard to find a single idea or thought that can contribute in any sort of way to the solution of this important problem, which does not appear at least *in nuce* during the eighteenth century." It means simply that, at the turning-point of modern psychology, — *i.e.*, with Herbart, — the problem of attention received explicit formulation, as a problem which every system of psychology has to face; and that modern psychologists since Herbart have set it in the forefront of their investigations, as the older psychologists did not.

"The description and explanation of the facts comprised under the familiar term 'attention' constitute one of the most formidable difficulties which the psychologist encounters in the whole course of his enquiry" (Külpe). It is, then, not surprising that the treatment of attention differs very considerably in the different systems of psychology. At the same time, we must not exaggerate this divergence of opinion. It has become fashionable to quote the phrase "quot homines tot sententiæ" in regard to theories of attention. But the same thing might be said of a great many scientific questions, both outside and inside of psychology, and its truth is not incompatible with the final accomplishment of a good deal of solid work. There are serious differences of opinion concerning the nature of the attentive consciousness: but much of the disagreement is due to one-

sidedness, and not to radical opposition of standpoint. Some theories are descriptive, to the neglect of explanation; some emphasise facilitation, some inhibition; some lay stress on the motor phenomena, some on the affective; and so forth. These views need not be mutually exclusive.

The Instructor should, of course, be familiar with the general discussions in Höfler (Psych., § 42), James (Principles, esp. i., ch. xi.), Külpe (Outlines, §§ 72–76), Lipps (Grundtats., chs. iv., vii.), Stumpf (Tonps., esp. i., § 4, 1; ii., § 22, 1), Volkmann (Psych., ii., § 114), Wundt (Phys. Psych., esp. ii., ch. xv., 2). Of the monographic literature the following will, perhaps, be found the most useful works: G. E. Müller, Zur Theorie der sinnlichen Aufmerksamkeit, Leipzig, 1873; T. Ribot, La psychologie de l'attention, Paris, 1889; L. L. Uhl, Attention: a Historical Summary of the Discussions concerning the Subject, Baltimore, 1890; A. Pilzecker, Die Lehre von der sinnlichen Aufmerksamkeit, München, 1889 (gives Müller's later views); H. E. Kohn, Zur Theorie der Aufmerksamkeit, Halle, 1894 (gives Benno Erdmann's views: to be read with Külpe's criticism, Zeits. f. Phil. u. phil. Kritik, cx., 1896, 26); A. J. Hamlin, Attention and Distraction, Amer. Jour. of Psych., viii., 1896, 3 (gives a classification and abstract of theories).

Two principal classifications of attention cross and recross in the literature. The one divides attention into 'sensible' and 'intellectual,' the other into 'voluntary' or 'active' and 'involuntary' or 'passive.' The former is a subdivision in terms of the contents or objects given in the attentive state; the latter a subdivision in terms of the conditions of attention, passive attention being an attention that is determined unequivocally, by a single stimulus or incentive, and active attention an attention determined equivocally, by a plurality of stimuli or incentives. Neither distinguishes specific kinds or distinct modes of attention itself.

On the question of *grades* or degrees of consciousness, see, besides the textbooks cited, J. Ward, art. Psychology, Encyc. Britannica, 9th ed., xx., 47; Külpe, *op. cit.*, 32; Helmholtz, Sensations of Tone, 62. Lipps' discussion, Grundtatsachen d. Seelenlebens, 1883, 29 ff., is especially noteworthy on the negative side.

For an elementary statement of the view of attention adopted in the text, see the author's Outline of Psych., 1899, 134 ff.

Question (1) This and the following Question need not be exhaustively answered.

The psychology of the eighteenth century is often spoken of as the 'faculty psychology,' for the reason that it attempted to explain all the various phenomena of mind by the assumption of different mental faculties. It postulated one or more fundamental forces of mind, and then proceeded to deduce therefrom a number

of special powers or forces. C. Wolff (1679–1754) posited a single original faculty, the *vis repræsentativa;* C. Bonnet (1720–1793) —a man who did good work upon the problem of attention—posited two ultimate powers, those of sense-perception and reflection; J. N. Tetens (1736–1805) believed in three separate faculties, ideation, feeling and desire, though he refers them all to a single *Seelenkraft.*

We may say in criticism: (1) that the faculty names are merely classificatory concepts; and that the subsumption under them of the ideas, feelings, impulses, etc., which are really given in introspection, does not help us in the least degree towards an understanding of these processes. A faculty psychology must, that is to say, be at best a merely descriptive psychology, and can never rise to the level of explanation. (2) But, further, the faculties, which as class-names are products of scientific abstraction, become changed in the faculty-systems into actual forces or powers, which are supposed to give rise to the separate ideas, feelings, etc. In other words, the faculty which, rightly defined, is incapable of affording explanation, is substantialised, and so made the ground of a wrong explanation. The first criticism charges the faculty psychology with impotence; the second charges it with seeking by false pretences to conceal its impotence.

Herbart did more than any one else to overthrow the doctrine of faculties, though he cannot be said to have killed it (*cf.* Lotze, Lipps, Höfler).

See Braunschweiger, *op. cit.*, 17 ff.; Wundt, Phys. Psych., i., 1893, 11 ff., 14 ff.; ii., 482 ff.; G. F. Stout, A Manual of Psychology, 1899, 103 ff.

(2) There are four uses that the student should distinguish.

(*a*) Mind itself, as a 'real being' or 'simple substance,' is endowed with self-activity or spontaneity. This usage belongs to metaphysics, and is wholly out of place in psychology, which knows nothing of real beings. Ebbinghaus, Psych., i., 11 ff.

(*b*) There is a specific process, a simple and elementary activity-experience, to be found in certain consciousnesses alongside of the other constituent elements, sensation and affection. Against this, see the author's Outline of Psych., 118 ff.

(*c*) There is to be found in certain consciousnesses (*e.g.*, in the

attentive) a 'feeling of activity' (including the feelings of activity proper and of passivity), which in experience is *sui generis* and unanalysable, but in structure is complex. *Cf.* Hamlin's critique of Wundt, *op. cit.*, pp. 24 ff.

(*d*) "Mental activity exists when (and so far as) process in consciousness is the direct outcome of previous process in consciousness." G. F. Stout, Analytic Psych., i., 1896, 148.

(3) It is important that the student give a correct answer here, since the phrase 'states of consciousness' or 'states of mind' is still current, in popular parlance and in certain psychologies, as the equivalent of 'consciousnesses' or 'complex conscious processes.' The word 'state,' as employed in the text, is the German *Zustand;* it designates the mode or form of existence which — if we may use the metaphor — the conscious processes of a given time are enjoying; their relative importance or relative obscurity in the total consciousness; their fatness or leanness. We speak of attention as a 'state of consciousness' just as we speak of muddiness as a 'state of the roads,' or of a man's affairs as 'being in a bad state.' The 'roads' and the 'affairs' are, obviously, different from the muddiness and the badness, — and that is the difference between the processes attended-to or attended-from, and attention itself.

A. Attention as a State of Consciousness. — The 'clearness' of a process is a synonym for its 'best state.' Clearness implies (*a*) a maximal discriminability or separability from other processes, and (*b*) a maximal reproductive value (value for memory, association, imagination, etc.). Hence every psychological experiment that aims at discrimination presupposes a perfect attention, which is also the precondition of effective mental work. — Külpe, Outlines, 37, 425.

A question arises here, which will be differently answered by different theories. "The processes attended-from are rendered less clear and distinct." Less clear and distinct than what? — (*a*) If we hold that the essence of attention is inhibition, the repression of irrelevant processes, it follows that the natural or normal state of a process, — say, a sensation, — is its attentive state. In that event, the phrase means: less clear and distinct than the processes attended-to, and therefore unnaturally or abnormally obscure. (*b*) If we hold, on the other hand, that the essence of attention is a positive reinforcement or

facilitation, then the natural state of a sensation must be the state of inattention. The phrase means, in this case: less clear than the processes attended-to, *i.e.*, only normally clear. (*c*) If, thirdly, we hold that attention implies both inhibition and facilitation, the natural state of a sensation will, again, be the state of inattention; but this state will lie somewhere between the extreme states, of elevation and depression, that characterise the contents of the attentive consciousness. The phrase will now mean: less clear and distinct, not only than the processes attended-to, but also than the processes given in the normal state of inattention. — The possibilities should be weighed from the genetic as well as from the systematic point of view.

The word 'rises' in law (7) is used technically, as the equivalent of the German *Anklingen*. The process attended-to 'comes to a head,' attains its full conscious value, more quickly than the others.

EXPERIMENT (1). *First Law.* — Puzzle pictures abound in the cheap magazines, though they are as a rule very crude. One of the best of those known to the author is a purporting diagram of the brain-convolutions issued by the Munich art-journal *Jugend* at the time of the Psychological Congress of 1896. The convolutions are made up of babies, intertwined in all sorts of postures. Some observers, if no suggestion is offered, fail to find the babies at all; others find them only after an appreciable time. When they are found, the diagram becomes fairly alive with them, and the brain-perception is reduced to a very bare and vague schema.

FIG. 43. — The three faces in the moon.

Of the same order are the three faces — the coarse full face, the foreshortened three-quarter face and the profile — that can be found on the disc of the full moon. The student should be asked to adduce further illustrations of the law, from other sense-departments.

Question (4) The following may be mentioned. (*a*) When an overtone is heard-out from a clang, by concentrated atten-

tion, the tone itself becomes clearer, the rest of the clang more indistinct. This is a famous and much discussed instance of the 'effect' of attention. (*b*) Fechner's experiment: two forks are held to the two ears, and the resulting tone can be localised in either ear by direction of the attention. The tone on the one side is here rendered more distinct, the tone on the other side depressed. (*c*) Helmholtz' assertion that he could control the phenomena of retinal rivalry by attention is accepted by Pilzecker (pp. 33 f.); *cf.* Stumpf, Tonps., i., 244. (*d*) The obtaining of the plastic effect with the stereoscope is a good illustration. When once the figures have fused, we lose sight of the irregularities and flecks and roughnesses that bothered us at first, and wonder that we could have failed to see the solid form. More striking still is the gradual attainment of the effect by successive instantaneous illuminations (electric spark) of the stereoscopic slide.

Question (5) The experiment intended is the familiar one of 'bringing out' sensations of pressure, warmth, cold, tickling, etc., by simple concentration of the attention upon some portion of the skin. It is best to choose for the experiment a part of the body that is under stimulation (ordinarily unnoticed) by clothing. For long-continued attention to, *e.g.*, the finger means vasomotor changes in the finger, which may give rise to tingling, pulsing, or what not. In such a case we have, not an enhancement of preëxistent sensations, but simply the observation of sensations which are due to the same conditions that have coöperated to produce the attentive state. — Pilzecker, 37 f.; D. Hack Tuke, Illustrations of the Influence of the Mind upon the Body, etc., 1884, (2d ed.) i., 33 f.; H. Maudsley, The Physiology of Mind, 1876, 316 f.; G. A. Tawney, Philos. Stud., xiii., 1897, 203 f.

EXPERIMENT (2). *Second Law.* — (*a*) Not only is the selected component intensified; it is possible, by successive attentions, to construct a melody from the separate tones, while the whole chord sounds on as accompaniment. Stumpf, Tonps., ii., 290. (*b*) The overtone is intensified; the fundamental, not. An attentive running-up of the series of overtones may bring out a high overtone which, with discursive attention, remained unob-

served. Melodies may be constructed, as before: 291 f. (*c*) No one of the three tones is intensified: 293 f.

This intensification must be very carefully distinguished from the gain in clearness and distinctness which falls under the first law. It occurs only in the case of intrinsically weak sensations. Stumpf gives the following instances, besides those selected for the Experiment. (*d*) Singing in the ear. "While the subjective tone was rising or disappearing, it could be unmistakably intensified by the direction of attention upon it. It became stronger, not merely clearer, more discriminable: it was clear and readily distinguished in the first moment of remarking. I could induce the intensification not only by an action within the ear, which made itself known by a muscle-sensation and at the same time somewhat raised the pitch of the tone, but also without the action and the rise in pitch. This latter mode of intensification was, however, possible only with a very weak intensity of the tone" (i., 373 f., 427). The author can confirm this experience. (*e*) Noises which contain tones, *e.g.*, the noise of the train as heard in a sleeping-car: ii., 292 f. This observation is easily verified. (*f*) Difference-tones: ii., 292, 354. The observation requires more practice than the preceding.

It seems to follow that the positive element in attention (the reinforcement or facilitation) is the condition solely of the increased clearness of the process attended-to; whereas the negative element (the inhibition) is responsible for an intensification of weak processes. For the intensification is most easily accounted for as a rise of the weak sensation, by the removal of counter-influences in the nervous system, to the full (or approximately the full) intensity which it would have possessed in its own right had those counter-influences been absent (Stumpf, i., 72, 374; ii., 293). The teleological significance of the arrangement is evident; with any other, a reliable series of judgments of intensity or of intensive differences would, so far as we can see, have been impossible. A maximal degree of attention is, in actual fact, the *sine qua non* of accurate judgment. But, if attention exerted an intensifying effect, the weak or moderately intensive sound would be strengthened by the very act of observation; the attentive following of a *diminuendo* would be impossible. The facts indicate, further, that intensive change is not the essential feature of the attentive state. That is rather to be sought in the clearness and permanence-for-judgment of the objects of attention (i., 72; ii., 307; *cf.* ii., 277 ff.).

It need hardly be said that the phenomena of attentive intensification are not confined to the sense of hearing. They can, however, be there observed in exceptionally pure form.

Cf., further, G. T. Fechner, In Sachen d. Psychophysik, 1877, 85 f.; Revision d. Hauptpuncte d. Psychophysik, 1882, 270 f.; and *ct.* Münsterberg, Psych. Rev., i., 1894, 39 with Hamlin, *op. cit.*

Questions (6), (7) These Questions may be similarly answered. A process is intensified, when it is intrinsically very

weak; a process is lengthened, when it is intrinsically very short. Instances of the third law occur in all experiments with stimuli of brief duration, — the time-value of 'brief' varying, of course, from sense-department to sense-department.

"It seems proper," says Külpe (Outlines, 432), "to distinguish between a change of sensations and sensation-attributes, and a change of their reproductory activity. All the effects of attention appear to fall under one or other of these rubrics." Again (429), "A change in the attributes and relations of sensations themselves is necessarily confined within certain narrow limits, whereas there is hardly any restriction upon change of judgment, *i.e.*, of reproduction. At the same time, we cannot admit, — what has often been maintained, — that a change of the former kind is wholly impossible." And (430), "In any case, attention produces its maximal effect in the reproductory sphere."

Except that he inclines to coördinate clearness (a direct change in the relations of sensations) with permanence-for-judgment (a reproductory effect), the author can subscribe to these statements. It is extremely tempting to assert, off-hand, that attention increases both the intensity and the duration of the process attended-to. But we have seen that there is no evidence of intensification in the case of intrinsically strong sensations; and that the intensification of weak sensations is rather a coming of the processes to their intensive rights, than a positive reinforcement of their intensities. The same thing holds, in the author's judgment, of duration. Attention cannot lengthen an intrinsically durable process. On the other hand, it helps intrinsically transient processes to their full conscious effect, by removing counter-influences that would tend to swamp them; it lengthens brief processes in precisely the same way that it strengthens weak processes. The teleology of this is, again, evident. With any other arrangement, exact judgments of durations or of temporal differences would be impossible.

The author knows of no very satisfactory way to demonstrate directly the truth of this third law. If *O* is subjected to a slow succession of brief stimuli (noises of moderate intensity, or brief flashes of weak light from a pierced disc revolving before a window in the wall of the dark room), and his attention diverted from these stimuli for a little time by conversation or directions, it is often possible to evoke later on the (attentive) judgment: "They seem, now that I look at them or listen to them, to last longer than they did just now, when you were talking to me." If this pronouncement come without suggestion, it may be accepted as evidence of the law. It must, of course, be made of the series at large, and not of particular terms in the series. — Indirect evidence is afforded by the fulness of *O*'s introspection, the number of characteristics that he has remarked, in the attentive state, as compared with its poverty in the state of inattention.

This attentive lengthening of a simple impression must by no means be confused either with the permanence-for-judgment (reproductory permanence) referred to above, or with what is called the 'inertia of attention' ("the atten-

tion holds fast to something already given more easily than it finds something that has to be looked for"). See Stumpf, Tonps., i., 244 f., 386, 391; ii., 318, 358; Fechner, Abh. d. kgl. sächs. Ges. d. Wiss., vii., 395; Revision d. Hauptpuncte d. Psychophysik, 1882, 283.

Question (8) The fourth law is borne out by the verdict of introspection in all cases of attentive observation. When one has found the puzzle-figure or the overtone, and is attending to it, the rest of the puzzle-picture and the rest of the clang do not stand out in a middle degree of clearness above the sights, sounds, etc., of one's surroundings; they are as indistinct and obscure as these surroundings. If one has singled out two overtones, by the attention, these two tones stand with equal clearness in the foreground of consciousness: one cannot hear the one more clearly than the other, and sense both more clearly than the remaining processes in consciousness. Hold the two hands to the ears, and rub together the forefinger and thumb of each hand. You can divide the attention equally (though not for any length of time) between the two noises; you cannot distribute it more to the one than to the other, and to both more than to what is, *e.g.*, before your eyes. The rule holds in every case: while "we are compelled by certain facts of the mental life to speak of at least two different states of consciousness, which may vary in degree" (Külpe), introspection never reveals to us more than two states in a given consciousness, no matter what the degrees of clearness or obscurity may be. Pass beyond the second state, and you come to the unconscious, *i.e.*, psychologically, to nothing.

Great care must be taken, in observations of the kind here described, to avoid an oscillation of the attention from contents to contents. Such oscillation is, as we shall see (in the meantime, *cf.* Stumpf, ii., 317), characteristic of attention in general; it may escape an untrained introspection, and thus give rise to the illusion of three grades of conscious clearness.

EXPERIMENT (3). *Fifth Law.* — In its classical form (Urbantschitsch), this experiment is performed with the ticking of a watch as stimulus. *O* is seated sidewise to the length of a corridor or large room. He may, if he desire, plug the ear which is not to be used for observation. *E* draws a chalk line upon the floor, from a point immediately below *O*'s ear to a point

some 8 to 10 m. distant. The watch is moved out, along this line and at the level of *O*'s ear, until the noise of its ticking is but just supraliminal: if the tick is very loud, the watch should be wrapped in a cloth. The resulting intermittences of sound are clear and very striking.

This experiment is sufficient to prove the fact of fluctuation, and may, perhaps, be given by way of introduction to that of the text. *O* must, in any case, be put through a course of practice, lasting at least as long as the experiment itself, with the Masson disc: the kymograph need then be used only for the experiment proper. Sustained effort of observation, and a mechanisation of the hand-movements, are essential to the obtaining of valid results.

PRELIMINARIES. — The drum should be set, if the mechanism of the kymograph permits, for revolution once in 60 to 100 sec. When longer times are taken, *O* (at any rate, in this stage of practice) grows inattentive, and the results are therefore untrustworthy.

Note that, in the arrangement recommended in the text, the crests of the curve of fluctuation represent disappearances, and the valleys reappearances of the grey ring. This mode of reaction is, for most *O*'s, preferable to that in which the bulb is pressed at reappearance of the sensation.

Lange noted the points of maximal sensation intensity; Münsterberg (85) insists that the moment of disappearance is more certainly remarked. Eckener required his *O*'s to raise the finger at disappearance, and lower it at reappearance (359); Pace had the button of a reaction-key pressed at disappearance, and released at reappearance (391). In Lehmann's investigation, the rubber bulb was pressed when the minimal sensation changed (76).

Question (9) The length of the attention wave, as registered in these experiments, is extremely variable. It will probably amount to 6 or 8 sec., though it may rise as high as 18 or 20 sec. The author has records in which waves of 2 and of 24 sec. duration occur. Such extreme times are suspicious; indeed, in the great majority of cases, they are thrown out by the introspective account. See Questions (11), (12).

(10) It will probably be found that the time of disappearance is considerably shorter than the time of appearance. The rela-

tion varies with the character of the stimulus-difference. The more nearly liminal this is, — the more nearly the grey resembles the white, — the longer, proportionately, are the times of disappearance. The clearer, greyer, the grey, the shorter are the disappearances.

The relation can be more easily traced if the periods in question are sharply marked upon the fluctuation curve. This may be effected by substituting for the rubber bulb a pneumatic reaction-key. *O* holds the button of the key down, during the absence of the grey, and lets it spring up again when the grey returns. The curve thus runs at two levels: the upper lines represent the periods of disappearance, the lower the periods of persistence in consciousness.

Minimal stimuli or stimulus-differences are chosen for the reason that any blurring or indistinctness of the corresponding sensations or sensation-differences will mean their complete disappearance. It is far easier to say that we do or do not hear or see something than it is to be sure that what we see or hear has grown more or less clear. Indeed, the attention seems to be no more able (if we may use the expression) to induce fluctuations upon a continuous intensive stimulus than it is to lengthen or strengthen an intensive sensation.[1] *Cf.* Marbe, 636.

(11) Objective sources of error, inherent in the mode of registration, may be neglected. Important are: (*a*) inattention, (*b*) distraction, (*c*) maladjustment of the peripheral organ. *O* must give his full and (though the phrase is really a contradiction in terms) his continuous attention to the stimulus. Otherwise, the fluctuations of the grey ring will indicate, not crests and valleys of the attention wave, but alternations of attention to stimulus and attention to the ideas constituting the consciousness of the time. Secondly, disturbing stimuli must, so far as possible, be ruled out. At the best, however, the movements of breathing, the rustling of clothes, etc., remain as possibly

[1] This does not mean, of course, that we can hold a bright light or a strong sound steadily before consciousness, by continuous attention. Attention is intrinsically intermittent. What happens is, that the intensive impression remains unchanged until it is presently relegated to the background of consciousness by some intruding (or rather, relieving) impression or idea. So long as we *are* concentrating our attention upon it, it does not show the oscillations that are characteristic of minimal stimuli.

distracting stimuli. Thirdly, an unsteady fixation or a change of accommodation may bring the grey ring to disappearance.

Fortunately, a trained *O* is able by introspection to discriminate between these accidental or artificial oscillations and the true fluctuations of attention. And the quantitative procedure comes, here as so often, to the aid of qualitative analysis. On the one hand, by direct comparison of the kymograph curve with *O*'s written record, *E* can identify the 'good' and the 'bad' fluctuations; on the other, this record is made more careful and more reliable by *O*'s knowledge that the curve will bear him out in the truth and detect him in falsehood. *Cf.* what was said above of the plethysmographic curves, p. 181.

(12) This question raises the whole problem of the fluctuations of attention. The best general account is that given by Wundt, Phys. Psych., ii., 295–301. A programme for investigation may be made out as follows.

(*a*) *Variation of Stimuli.* — For sight, it is probably best to use two Masson discs, the one showing grey on white (black radius), the other grey on black (white radius). For sound, we may take the ticking of a watch, or the continuous hiss of the flame of a Bunsen burner: if the experiments are performed very early in the morning or late at night, the gas-pressure will be constant. For touch, some form of the interrupted current may be employed (Lange, 401; Lehmann, 77). — In every case, the intensity of stimuli and, in the case of sight, the magnitude of the stimulus-difference should be varied (Marbe, 622, 624).

(*b*) *Variation of Registration.* — We have noted two modes of registration: a continuous following of the course of attention by pressure on the rubber bulb, and a sharp demarcation of periods of disappearance from periods of disappearance by pressure on a pneumatic reaction-key. To these may be added a registration of maximal appearances only, *i.e.*, a determination of the highest point on the crest of each attention wave. The pneumatic key will serve here; or recourse may be had to an ordinary key and an electro-magnetic marker.

[N. Lange, who was the first systematically to investigate the fluctuations of attention, and who worked by the last-named method, found that the attention period (the time-interval from

maximum to maximum of sensation) was but little variable within a given sense-department. His values were; for sight, 3.4 sec., for sound, 3.8 sec., and for electrical-cutaneous impressions, 2.5 sec. In view of the results of later observers, both the regularity and the smallness of these times call for explanation. Wundt (296) accounts for them partly in terms of method (the attention adjusts itself more easily and regularly to the stimuli), and partly in terms of stimulus intensity (choice of just clearly supraliminal values): *cf.* Marbe, 622, 632. Eckener (375) suggests the influence of a preconceived theory; Münsterberg (111), the rhythm of respiration. Lehmann (69) points out that his own method (that of the text) favours a continuous attention-strain, whereas Lange's method favours a pulsation of attentions, a succession of tensions and relaxations. The two methods are, therefore, directed upon different phenomena. Lehmann does not attempt an explanation whether of the extreme regularity of Lange's times or of the times themselves.]

(*c*) *Regulation of Stimulus.* — Helmholtz remarked, in experiments with a Masson disc, that the just noticeable grey does not remain constant; on the contrary, as the experiment proceeds, greys become visible which at first were unnoticed (Phys. Optik, 1st ed., 314 f.; 2d ed., 391). Pace (391) found that the fluctuations of attention, with a constant stimulus, are abrupt at the beginning of an experimental series, gradual towards its close. It follows from these observations that experiments should be made during which the stimulus remains not objectively but subjectively constant. The conditions are fulfilled if we employ a Masson disc under such circumstances that its two or three outermost 'grey rings' are at first imperceptible, and direct *O* to shift his fixation-point to these rings as they become successively visible.

[Pace (394), working in this way, found a fluctuation period of 3.5 sec., with a small *m. v.* Wundt (296 f.) notices the correspondence of this time with the value obtained by N. Lange. The agreement is, indeed, most striking; but Lange's results are not explained by it.]

(*d*) *Variation of State of the Sense-organ.* — Pace (399 f.) found that paralysis of the muscles of accommodation by a 1 %

solution of Homatropinum hydrobromicum left the fluctuations unaffected. Urbantschitsch and Eckener (360) note that fluctuations are observed by ears lacking a tympanic membrane.

(*e*) *Simultaneous Registration of the Breathing Curve.* — Lehmann (76, 79) shows that there is a close connection between fluctuation of attention and breathing rate in the case of cutaneous stimuli. *Cf.* Wundt, 297 f.

(*f*) *Duplication of Stimulus.* — Eckener (368) performed experiments with two simultaneous stimuli from the same sense-department (*e.g.*, watch-tick and Bunsen burner); Lange (400) employed two disparate stimuli (watch-tick and Masson disc). *Cf.* Wundt, 299 f.

(*g*) *Observation of Memory Images.* — Lange (409 f.) noticed a fluctuation of memory images, as well as of peripheral sensation; and regards the memory image as the essential part of the mechanism of fluctuation. Eckener (370, 379) further investigated the fluctuation of the memory image. *Cf.* Fechner on the oscillation of the memory after-image: Elemente d. Psychophysik, 1889, ii., 493; and see p. 43 above, on the oscillation of after-images proper.

(*h*) *Introspection.* — It is very desirable that some one should do for these fluctuations what Bolton and Meumann have done for the phenomena of subjective accentuation: secure and publish full introspective reports of the course of consciousness during the experiment. Münsterberg condemns Lange's results *in toto* (82 f.); Lehmann (74 f.) thinks that Eckener's rarely occurring 'objective' fluctuations (361) are the fluctuations to be observed, and that his 'subjective' fluctuations are simply matters of inattention; Wundt (301) believes that Eckener's subjective fluctuations are identical with those that Lehmann registered; and the author, who can confirm Eckener's observations, is of the same opinion. In view of the acknowledged competency of introspection in this field (Wundt, 299; Münsterberg, 86; Eckener, 362; Pace, 401; Lehmann, 74), and of the refinement now attained by the method of registration, it would seem advisable to make a systematic appeal to introspection for the settlement of the disputed issues.

Literature. — V. Masson, Comptes rendus, xviii., 1844, 289;

Ann. de Chimie et de Physique, 3me sér., xiv., 1845, 129; Fechner, Helmholtz, Wundt, as quoted; V. Urbantschitsch, Centralblatt f. d. medic. Wiss., 1875, 626; Pflüger's Arch., xxiv., 1881, 574; xxvii., 1882, 440; N. Lange, Philos. Studien, iv., 1888, 390; H. Münsterberg, Beiträge z. exp. Psychologie, ii., 1889, 69; H. Eckener, Philos. Studien, viii., 1893, 343; E. Pace, *ibid.*, 388; K. Marbe, *ibid.*, 615; A. Lehmann, *ibid.*, ix., 1894, 66; A. Pilzecker, *op. cit.*, 55 ff. W. Heinrich has recently asserted that pure tones do not fluctuate: see H. O. Cook, Amer. Journ. of Psych., xi., 1899, 119, 436.

(13) This question must be answered from the literature. It falls into two part questions: (*a*) Is the seat of the fluctuations central or peripheral? and (*b*): In the former event, are the fluctuations attributable to the mechanism of attention or to some other central mechanism? Münsterberg and Heinrich declare for a peripheral seat, and so close the second question. Marbe and Lehmann declare for the centre, but not overtly for the attention. Pace, and still more forcibly Eckener, refer the phenomena to the attention.

See W. Heinrich, Die moderne physiologische Psychologie in Deutschland, 2d ed., Zurich, 1899.

EXPERIMENT (4). *Sixth Law.* — The experiment upon the range of attention is best performed with visual stimuli, "because visual impressions can most easily be selected with a view to their apprehension as independent ideas" (Wundt, Human and Animal Psych., 241; Phys. Psych., ii., 287). Two methods have been employed for the determination. The first is that of instantaneous illumination (Dove, Zöllner, Helmholtz); an electric spark is flashed before the object-card, in a dark room, and *O* is required to describe what he has seen (Helmholtz, Phys. Optik, 710). This method has fallen into disuse. The second method, that of the tachistoscope, is better adapted to general laboratory purposes. The tachistoscope consists, in principle, of a falling screen or shutter which, in dropping or opening, exposes the object-card for a brief and accurately variable time.

The name 'tachistoscope' was suggested by A. W. Volkmann (Sitzungsber. d. kgl. sächs. Ges. d. Wiss., 1859, 90). One of the best known demon-

strational forms of the instrument is Wundt's fall-chronometer, figured in the Phys. Psych., ii., 291; H. and A. Psych., 242. It consists of an upright back of blackened wood, 2 m. high, furnished with lateral guides, in which the screen runs. The screen is held up by a spring catch, and carries a white fixation-mark upon its lower surface. When the spring is released, the screen drops, and in falling exposes an object-card, 33 by 33 cm., upon which are 4 rows of letters or figures, about 6 by 4 cm. The card is completely re-covered when the screen comes to rest. The upper line of letters is exposed for about 0.09 sec., the lowest line for about 0.07 sec., and the middle lines for some 0.08 sec. The observers sit at a distance of 2 to 3 m.

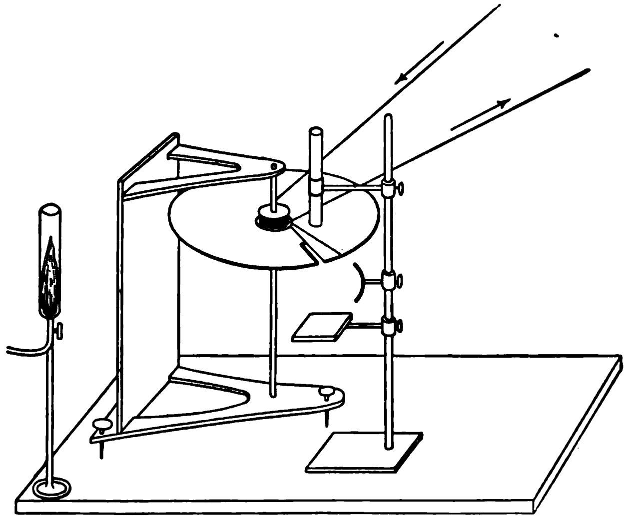


FIG. 44. — The apparatus of Goldscheider and Müller.

A smaller and more exact fall-chronometer is described by J. McK. Cattell, Philos. Stud., iii., 1886, 97, 307; Brain, viii., 1885, 295; *cf.* J. Zeitler, Philos. Stud., xvi., 1900, 381. A pendulum tachistoscope is figured by Wundt, Phys. Psych., ii., 334 (improved form in Zimmermann's catalogue, 1897, 8). The arrangement of the text, in which the rotating disc is made to do duty as a tachistoscope, is practically that of A. Goldscheider and R. F. Müller, Zeits. f. klin. Medizin, xxiii., 1893, 134. This, in turn, may probably be regarded as a simplified form of the apparatus devised by Helmholtz (Phys. Optik, 514), and used by N. Baxt (Pflüger's Arch., iv., 1871, 325) and S. Exner (Sitzungsber. d. wien. Akad., lviii., 2, 1868, 601). It is clear that the essential parts of the rhythm apparatus (p. 349) can be employed for the experiment. As the same parts can be used again, later on, in the Association experiment, the author has recommended the disc and motor in preference to a fall-chronometer. A cheap form of the latter is described in his Primer of Psych., 92 f.

MATERIALS. — The dimensions given in the text are those which the author has employed. Length of tube, size of disc, etc., may be varied as convenience suggests.

Since the disc is to stand vertically, the movable sector must be cut to give an even balance. Let the central circle, to which the sector is attached, have a radius of 10 cm., and continue the counter-sector outwards, on the opposite side, for 5 cm.

If the experiment is to be carried beyond its first stage, object-cards must be prepared in which the simple figures are replaced by numerals and letters (sense and nonsense arrangements). These must, in some cases, be arranged upon different lines: short words, *e.g.*, are best arranged in three lines. The simpler figures may also be arranged in patterns, which occupy more than one line upon the card. The time of exposure may then be correspondingly increased to 0.04, 0.06, etc., sec.

If one row of figures, filling the field vertically, is exposed, the time of exposure, T, is twice the time required for the sector to pass a point: *i.e.*, $T = 2\,R\left(\frac{S}{360}\right)$, where R is the rate of revolution, and S is the angular magnitude of the sector. Thus, for 1 revolution in the 1 sec., and an opening in the disc of 3.6°, we have $T = 2\left(\frac{3.6}{360}\right) = .02$ sec. If several rows of figures are shown, this proposition does not hold. Thus, with a sector of 10.8° and 3 lines of figures, the time of exposure of each line is .04 sec.

Let t be the time required for the sector to pass a point — $t = R\left(\frac{S}{360}\right)$ — and let h_1 and h_2 be the heights, respectively, of the total exposure field, and of the portion of it (figures or lines) whose time, T_1, is to be determined.[1] Then $T_1 = t + \left(t \div \frac{h_1}{h_2}\right)$; $T_1 = t + \frac{th_2}{h_1} = t\left(1 + \frac{h_2}{h_1}\right)$. It is important that this formula be understood; *cf.* the instance worked out by Goldscheider and Müller, 154 f.

Note that, in any case, the exposure time is not by any means coincident with the excitation time. The after-effect of stimulation in the visual apparatus is of considerable duration. See answer to Question (2), below.

[1] The values h_1 and h_2 may be found by direct measurement; h_1 may also be obtained by the following geometrical process. The apparatus furnishes two similar triangles, whose common vertex is the observing eye. The base of the one triangle is the width of the open sector (taken at the left side of the lumen of the tube), and that of the other triangle is h_1. Then the side of the first triangle is to the side of the second triangle as the sector opening is to h_1.

For words and letters, the type known as grotesque or gothic (without serifs and hair-lines) should be employed. *Cf.* E. C. Sanford, Amer. Journ. of Psych., i., 1888, 424.

PRELIMINARIES. — *O*'s unused eye must be closed as in the campimetrical experiments.

Question (14) The results vary, according to the complexity, familiarity, 'meaning' of the figures. On the average, it is safe to put the maximal range of attention at 4 to 5 simple impressions (lines, letters, numerals), and at about three times this number of word-elements (2 or 3 short monosyllabic words). See Cattell, Philos. Studien, iii., 1886, 126 f.; Wundt, Phys. Psych., ii., 287 f.; Philos. Studien, xv., 1900, 311; Goldscheider and Müller, esp. 135, 142 f., 151 f., 154; B. Erdmann and R. Dodge, Psychologische Untersuchungen über das Lesen, auf experimenteller Grundlage, Halle, 1898, 137, 140; Zeitler, 412.

The method employed in the experiment is the most favourable possible to a wide range of attention. The number of figures increases regularly from test to test; and the exposure of the figures is repeated, at 1 sec. intervals, until final judgment is passed. The former of these rules means that *O* enters upon the successive experiments with a definite and definitely directed expectation; the second means that his 'mental preparation' for the impressions which he finally apperceives is of a very high order.[1]

An 'objective' *O* does not abuse these advantages. If, however, *E* has any reason to suppose that the suggestions of the method are too strong for accuracy of result, he must take one or two sets of cards in haphazard order, and so check the 'procedure with knowledge' by a procedure 'without knowledge.' *Cf.* Zeitler's criticism, 422 ff.

(15) This question is answered by Wundt as follows.

(*a*) The duration of stimulus must be short enough to preclude eye movements.

(*b*) The stimulus must be of so limited an extent, and its position so accurately defined by the fixation-mark which is shown before exposure, that all of its constituents can be seen with approximately equal distinctness, *i.e.*, that the total image falls, to all intents and purposes, within the area of direct vision.

[1] It does not mean, for introspection, that a roving of the attention occurs, so that the various parts of the field, distinctly seen in successive exposures, presently fit themselves together. *O* is clear upon the point that the final judgment is a judgment of simultaneous apprehension. See answer to Question 17 (*c*), below; and *cf.* Zeitler, 393 f.

In consideration of the special requirements of the present experiment, we may add that the number of objects thus offered to direct vision must be greater than the number that can be simultaneously apperceived.

(*c*) The time of exposure must begin at the same moment over all parts of the exposed field. Or, at any rate, there must be no noticeable time-differences in the illumination of the various regions.

(*a*) The state of retinal adaptation must be as favourable as possible. Especially must sudden transitions from dark to light be avoided.

(*e*) Persistent after-images of the exposed stimuli must be ruled out.

(*f*) The time of exposure must be short enough to preclude the roving of attention from one part to another of the exposed field.

(*g*) Provision must be made, in arranging the apparatus, for the giving of a ready-signal at the proper interval before exposure. Philos. Stud., xv., 289.

(16) One of the requirements of a good instrument is that there shall be no noticeable time-differences in the illumination of the various parts of the field. We may take this to imply that the mode of exposure is indifferent, provided that there is no introspective evidence of roving of attention, and no chance given for eye movements. And this is Wundt's view: Philos. Studien, xv., 291 ff., 303; xvi., 1900, 65. Goldscheider and Müller think that, if there is a subsequent roving of attention, the mode of exposure determines the course of apperception, even though the exposure itself were regarded as instantaneous (154 f.). Erdmann and Dodge, on the other hand, emphasise the necessity of a simultaneous exposure (Ueber das Lesen, 94 ff.; Zeits. f. Psych., xxii., 1900, 243). It would be well worth while to re-investigate the roving of attention under exact conditions: *cf.* Zeitler, 404; Dodge, Psych. Rev., viii., 1901, 56; Wundt, Völkerpsych., i., 1, 1900, 540 ff.

(17) The experiments with letters, numerals and words may suggest themselves. *E* may also wish to compare the result of successive minimal exposures with that of a single wider exposure. The carrying-over of the experiment to another sense-

department, *e.g.*, to sound, changes the character of the enquiry. See Wundt, Phys. Psych., ii., 288, 292, on the question of the range of consciousness.

ADDITIONAL QUESTIONS. — (17 *a*) Why has the electric-spark method been abandoned?

The disturbance of adaptation (*a*) impairs the objective apprehension of the stimuli, and (*b*) hinders introspection of the perceptive process. Wundt, Philos. Studien, xv., 301.

(17 *b*) How long may the exposure be made, consistently with the avoidance of eye movement?

At least .25 sec. Wundt, 307, and the preceding discussion.

(17 *c*) What is the 'roving' of attention? Is introspection able to detect it?

The attention may travel from part to part of a total idea or perception. Suppose, *e.g.*, that the figures of the object-card are retained as after-image or memory after-image. Then the attention may turn successively from figure to figure, although the time of exposure lay well within the eye movement limit. Wundt, 309 f. — Introspection detects the roving. Groups of 5 or 6 numerals, *e.g.*, are apprehended in two parts or halves; groups of words in part-groups. *Cf.* M. Friedrich, Philos. Stud., i., 1883, 66 f.; but see also the refs. under (16) above.

(17 *d*) How far does our arrangement satisfy the requirements of a good tachistoscope? — Answer from Question (15).

EXPERIMENT (5). *Seventh Law.* — *Materials.* — There is, of course, no guarantee that the gearing of the metronome clockwork is so accurate that the ring and its stroke are precisely simultaneous. The author is accustomed to test the instrument as follows. (*a*) Remove the floor of the clock chamber, turn the metronome upside-down, and move the pendulum very slowly to and fro. If you detect any discrepancy between the ring and the stroke, at any one of the possible settings of the bell adjustment, reject the instrument. (*b*) Having selected a metronome, set the pendulum vibrating at 144 or 152 to the 1 min., and let two practised *O*'s (Instructors, or students who have worked with other appliances) listen to the series of sounds, the one concentrating his attention upon the succession of strokes, and the other upon the succession of rings. If the former is able to

hear the stroke before the ring, and the latter to hear the ring before the stroke, the instrument may be considered available.

The stroke should be 'thinned' for the experiment by removing the floor of the clock chamber and mounting the metronome on a layer of felt: *cf.* p. 175 of the text.

EXPERIMENT. — (*a*) It is natural, in this case, that the attention follow the strokes, as the more frequent and insistent impressions. Hence, while it is not difficult to hear ring and stroke together, *O* will probably say (without any suggestion or direction from the Instructor) that the ring comes later than the stroke. He should then try, by voluntary effort, to realise the three possibilities: simultaneity, bell earlier, bell later. The observation 'bell earlier' will be occasional and intermittent, if it occur at all. (*b*) In this case, the rings come at short intervals, so that they form as obvious and distinct a series as the strokes. *O* will probably declare for simultaneity. Purposed direction of attention, however, will readily change his judgment to 'bell earlier' or 'bell later.' Note that, in the former event, the stroke is very indistinct; it is swamped in the 'fall' of the bell-tones; and that, when once the attention has taken a definite direction, it is not easy to change from this to the other ('inertia' of attention).

Question (18) The seventh law is valid.

(19) The experiments on attentional time-displacement form one of the most interesting and most difficult chapters of experimental psychology. They are intimately connected — by way of the eye-and-ear observations of astronomy — with the simple reaction experiment: see the references on the history of the simple reaction, p. 213.

The instrument employed in the classical investigations of the subject is the complication pendulum. The earlier and cruder form of this apparatus (1861) is figured by Wundt, Human and Animal Psych., 270; a more elaborate form, which allows of the simultaneous release of a visual impression, a bell-stroke, a cutaneous pressure, and one or more cutaneous electrical stimuli, is figured in the Phys. Psych., ii., 405.

On the complication method, see Wundt, Philos. Studien., i., 1881, 34 f.; xv., 1900, 579; on the defects of the current instruments, Mind, N. S., ix., 1900, 287. For experimental results, see W. von Tschisch, Philos. Studien,

ii., 1885, 603; C. D. Pflaum, *ibid.*, xv., 139. For modifications of method, see J. R. Angell and A. H. Pierce, Amer. Jour. of Psych., iv., 1892, 531 ff.; J. Jastrow, *ibid.*, v., 1892, 241.

An ingenious student will find no difficulty in reproducing Wundt's first pendulum apparatus, or in adapting the vernier chronoscope to the present experiment by arranging that, *e.g.*, an electric shock be given simultaneously with a sound or light stimulus. The instrument devised by Angell and Pierce is fairly easy of construction; but the author has not used it. Apparatus of the type of Sanford's pendulum circuit breaker (Amer. Jour. of Psych., vi., 1895, 581 ff.) may also be employed.

Question (20) The explanations given should be both psychophysical and psychogenetic. That is to say, we must account for the effect of the stimulus for attention both in terms of brain mechanics and in terms of the development of the organism.

The principal conditions are as follows.

(*a*) *High Intensity of Stimulus: or its Equivalent, Great Extension.* — The psychophysical process is here one of relatively great strength, and is therefore not easily suppressed by counter-excitations. — Pilzecker, 19; Külpe, 438.

(*b*) *Suddenness of Stimulus.* — This has two psychophysical reasons: the increased excitability of the nervous elements affected, due to their previous rest from stimulation of this especial kind; and the fact that excitations suddenly set up are not so much weakened as other excitations are by having part of their energy drained off into secondary nervous channels. — Pilzecker, 20. Long duration of stimulus, on the other hand, means a constantly increasing waste of energy by such drainage: Müller, 125 ff.; Pilzecker, 20; *cf.* Külpe, 438.

(*c*) *Connection of the Stimulus with the Present Contents of Consciousness.* — The more nearly the excitation correlated with a given stimulus coincides with an excitation now in progress within the psychophysical portion of the nervous system, the more easily will it make its way and the more dominant will it be. — Pilzecker, 19. This condition holds not only when the like excitation is actually in progress, but also when the excitation correlated with the stimulus coincides with a form of excitation habitual to the sensorium in question. "If our attention is directed upon an auditory stimulus," says Pilzecker, "auditory stimuli will be those that most readily come to consciousness." To which we may add: if we are acousticians or aurists by profession, auditory topics will at all times have a ready entry to our consciousness.

(*d*) *Novelty of Stimulus.* — 'Novelty' means, psychologically, 'non-associatedness.' The novel impression is the impression that lacks associative supplements in consciousness; that stands alone, in isolation. It is evident that such an impression, having no distracting impressions by its side, "can receive a measure of attention which is altogether impossible when it is accompanied or surrounded by a number of other objects of perception" (Külpe). This sentence is easily translated into psychophysical terms. — Here belong

also the influences of contours and of simultaneous contrast. Pilzecker, 20; Külpe, 438; Lipps, Suggestion u. Hypnose, 1898 (Sitzungsber. d. k. bayer. Akad. d. Wiss., 1897), 424.

(*e*) *Movement of Stimulus.* — The influence of movement is explained by the avoidance or reduction of fatigue in the parts of the organ stimulated. Pilzecker, 20; Stumpf, Tonps., ii., 337 ff.; Külpe, 300 f.

In all these cases, we have offered a proximate explanation of the effect of the condition. Such explanation must always be attempted, even in cases where (as in the instance of Movement, above) its inadequacy is patent from the outset. We may believe with Cattell (Psych. Rev., vii., 1900, 343) that "perceptions are... in large measure the result of experience and utility,"[1] but they must still have a psychophysical substrate, on the one hand; and, on the other, the bare reference to utility does not explain them.

When we enquire into the psychogenetic reasons for the value of the conditions named, we find that the conditions themselves fall into three groups. The first group includes intensity; extension ('voluminousness,' in James' phraseology); and suddenness and novelty, in so far as they are also reducible to intensity. Intensity appeals to the organism as organism: we cannot think of an organised being which should disregard intensive impressions in its environment, and yet survive. Even civilised man, with all his powers of educated self-restraint, 'starts' when he hears a loud sound, and has his eyes drawn irresistibly to a brilliant light. 'Connection with the present contents of consciousness' is a condition of a different order. It becomes effective at a much later stage of mental development than does intensity: at a stage when mind has passed beyond the inconsequence of sense-impression, and has reached the level of more or less continuous imagery. The third group of conditions includes movement, and suddenness and novelty in so far as these exert a specific influence, set up a definite affective reaction. The group may be termed, with James, the group of 'instinctive stimuli.' The "perception is one which, by reason of its nature rather than its mere force, appeals to some one of our normal congenital impulses, and has a directly exciting quality" (i.,

[1] *Cf.* what is said of the place of history in a theory of perception, p. 228 below.

417). Of the 'why' of this appeal we can say only that, given the course of development as we know it, the organism must have attended to movement, etc., in its surroundings, or have paid the penalty of inattention with its life. The moving, the new and the sudden are all possible — even probable — sources of danger. *Cf.* the author's Outline, 139, 275; Primer, 191.

B. The Sense-processes in Attention. — Question (21) The *locus classicus* for the strain-sensations in attention is Fechner, Elem. d. Psychophysik, 1889, ii., 475 (*cf.* 490 f.). The reader must, of course, make allowance for Fechner's terminology, and discount his identification of strain-sensation with strain of attention: the account was published in 1860.

"If we turn our attention from one sense-department to another, we have at the same time a definite *feeling* of the change of direction. The feeling is indescribable, but any one can readily reproduce it in experience. We may term it the feeling of a change of localisation of tension. We feel a forward direction of tension in the eyes or a lateral direction of tension in the ears, — a tension that increases with the degree of attention, — according as we are attentively fixating something or attentively listening to something; so that people speak commonly of a 'strain of the attention' itself. We feel the difference most clearly if we shift the direction of attention quickly back and forth between eye and ear. We get the same feeling, differently localised about the different sense-organs, when we are trying to taste, smell or touch anything with discrimination.

"But more: I have a feeling of tension, precisely analogous to that which I get with keen concentration of sight or hearing, when I am trying to envisage as clearly as possible some image of memory or of fancy; and this precisely similar feeling is very differently localised. With the keenest possible concentration upon external visual objects or upon after-images, the tension has an unmistakable forward direction; and if the attention is turned to other sense-departments, this direction changes, according to the position of the external sense-organs: but the rest of the head is free from all feeling of strain. When, on the other hand, memory or imagination is actively employed, the feeling of tension withdraws altogether from the external sense-organs, and seems rather to have its seat in that portion of the head which contains the brain. If I try to represent some scene or person to myself with especial vividness, the vividness of the representation depends, not upon the forward strain of the attention, but rather (if I may say so) upon its retraction inwards."

A hardly less well-known passage is that in James' Psychology, 1890, i., 300.

"Whenever my introspective glance succeeds in turning round quickly enough to catch one of these manifestations of spontaneity in the act, all it can ever feel distinctly is some bodily process, for the most part taking place within the head. . . .

"In the first place, the acts of attending, assenting, negating, making an effort, are all felt as movements of something in the head. In many cases it is possible to describe these movements quite exactly. In attending to either an idea or a sensation belonging to a particular sense-sphere, the movement is the adjustment of the sense-organ, felt as it occurs. I cannot think in visual terms, for example, without feeling a fluctuating play of pressures, convergences, divergences and accommodations in my eyeballs. The direction in which the object is conceived to lie determines the character of these movements, the feeling of which becomes, for my consciousness, identified with the manner in which I make myself ready to receive the visible thing. My brain appears to me as if all shot across with lines of direction, of which I have become conscious as my attention has shifted from one sense-organ to another, in passing to successive outer things, or in following trains of varying sense-ideas.

"When I try to remember or reflect, the movements in question, instead of being directed towards the periphery, seem to come from the periphery inwards and feel like a sort of *withdrawal* from the outer world. As far as I can detect, these feelings are due to an actual rolling outwards and upwards of the eyeballs, such as I believe occurs in me in sleep, and is the exact opposite of their action in fixating a physical thing. In reasoning, I find that I am apt to have a kind of vaguely localised diagram in my mind, with the various fractional objects of the thought disposed at particular points thereof; and the oscillations of my attention from one of them to another are most distinctly felt as alternations of direction in movements occurring inside the head."

The student should read also the next two paragraphs of p. 301, and the remarks on pp. 435 f. The passage quoted is a fine piece of introspection, marred only by the pictorial reference to the 'lines of direction' shooting across the brain.

Kohn (*op. cit.*, 48) remarks: "If we consider the proofs that James adduces for the presence of these feelings, we find that they speak not for such presence at all, but simply for the possibility of discovering these feelings by the direction of attention upon them." "If the feelings were present while the attention is directed upon some other object, there would be no need at all of the 'turning round' or the 'introspective glance.' We should be conscious of them without this." To which the obvious reply is, that we *are* conscious of them 'without this'; otherwise there would be no cue for introspection. We do not attempt to introspect the non-existent. But, when we are giving a psychological account of any contents, we examine it in the state of attention.

The strain-sensations are present in the margin of consciousness while we are attending to something else; when we set to work to describe these sensations for psychology, we attend to them.

As to the part played in attention by these strains and tensions, we must distinguish between the strains themselves and the sensations arising from them. Pilzecker (*op. cit.*, 40) is probably right when he says: "We must regard these motor phenomena not as mere accidental concomitant phenomena; they are rather an integral part of the mechanism of the sensory attention, and help to constitute and to maintain it." *Cf.* Wundt's law of the correspondence of apperception and fixation (Phys. Psych., ii., 108, 145). On the other hand, the strain-sensations, "in view of the great variety of sources from which they are derived, must be regarded not as constitutive, but only as consecutive characteristics of the state of attention. They stand guard over attention, so to speak, to prevent its too persistent occupation with a single object; and their growing unpleasantness is a warning signal of excess of function in some particular part of the nervous system, which must ultimately prove harmful to the organism" (Külpe, Outlines, 436).

(22) *Ct.* Helmholtz, Phys. Optik, 2d ed., 890 f., with Hering, Hermann's Hdbch., iii., 1, 1879, 548.

(23) No. Strain-sensations are, at best, a measure of the effort made in attending, not of the degree of attention given. It would be more nearly true, perhaps, to say that the intensity of the strain-sensations would afford an inverse measure of the degree of attention, since it is when we attend most easily that we attend best, and when we attend with the greatest effort that we attend worst. But this statement also requires qualification. *Ct.* the author's Outline, 146 ff. with Münsterberg, Beiträge, ii., 1889, 24 f.

C. Attention and Affective Process. — Question (24) References must suffice here: the Instructor will, naturally, answer the general question in terms of his own psychological system.

(*a*) Tonps., i., 68. (*b*) Phys. Psych., i., 588. (*c*) Principles, ii., 344 f. (*d*) Outlines, 439. (*e*) Outline, 156. (*f*) Psychologie, 263, 266. (*g*) Phys. Optik, 606.

EXPERIMENT XXVI

§ 44. **The Simple Reaction.** — The reaction experiment is introduced here solely for its 'qualitative' value. It gives *O* an opportunity to introspect the typical 'motive' — the impulse — under standard conditions; it trains him in the control of attention; it furnishes an introspective foundation upon which more complicated experiments may later be based; it is the key to a large and very important section of psychophysical literature. The author believes that this view of the experiment is justified by its results; and believes, further, that the treatment in the text, incomplete though it is (and at this stage must be) on the psychological as well as on the technical side, is in principle the right treatment. The details of theory and of method, passed over here without discussion, will be supplied in vol. ii. In the meantime, the Instructor may, of course, extend the experiment as far beyond the author's limits as time permits.

Questions (1), (2), (3). — These questions may be answered, most simply and directly, from the author's Primer of Psychology, 1899, ch. ix.

MATERIALS. — The vernier chronoscope is described by its inventor, E. C. Sanford, in the Amer. Journ. of Psych., ix., 1898, 191, and by the author in his Primer of Psychology, 182. It is cheap ($5.00), and works satisfactorily. The following are points not noted in the text.

The silk is ordinary 'button-hole twist.' It should be waxed over the length that is threaded through the bobs, to prevent wear by friction. The long pendulum should be slung in the red, and the short in the white silk: this arrangement makes the determinations easier.

The rod *b* is set cornering, that the threads may hang from an edge. The grooves in the surface of *b* prevent the threads from spreading, and thus altering the pendulum length.

The keys are set upon posts, in order that they may have a capacity of vertical adjustment, and so be brought into their right relation to the pendulums. The keys must be so set as to release the bobs with as little independent motion as possible. The threads of the two pendulums should lie in approximately the same plane when the face-hooks are caught in the hooks *f*.

The upper bar of the key turns on the screw as a pivot. The screw may be tightened or loosened as the working of the bar demands. The spring, which acts upon the downward projecting cam, holds the key either closed or

open. A slight lift of the spring (*e.g.*, with a screw-driver) on the button-side of the cam facilitates closing; a similar lift on the lips-side facilitates opening.

Step (*d*) may be omitted; and Sanford recommends that the student in every case proceed at once from (*c*) to (*e*): "the short pendulum is better regulated by the long one than directly by the watch." This is not the author's experience, though Sanford's method presents no especial difficulty. In counting swings, as in counting beats (p. 37 of the text), one must either begin with 'nought,' or throw off one at the end of the count.

In most cases, the chronoscope can be set up during a single laboratory period; the required length of the pendulums is suggested by the base steps. If, however, the Instructor sees that time is likely to be wasted in the attempt to make the units exact, he may advise that rough determinations be accepted, and an arithmetical correction introduced into the results. "Suppose, *e.g.*, that the long pendulum is found to swing 148 times in 2 min., and that coincidences occur every 37 swings. This will mean that the time of a single swing of the long pendulum is 0.81 sec., and that the short pendulum makes 38 swings to 37 of the long. The unit of the instrument is then 0.0213 ($0.81 \div 38 = 0.0213$), instead of 0.02, as it would be if exactly adjusted. In this case the final records would be reduced to hundredths of a second by multiplying by 2.13 instead of by 2" (Sanford).

Some *O*'s have a tendency to count short, *i.e.*, to record coincidence before it has been reached. This tendency must be controlled and corrected by the Instructor. It is better for inexperienced *O*'s in general to count too far (to one or two beyond coincidence); the point of coincidence is then certainly attained, and the extra swings can easily be thrown off. In cases of doubt between two swings, the mean must be taken (*e.g.*, 'coincidence at both 10 and 11' must be recorded as 'coincidence at 10.5'). But exact counting is, in reality, a much easier matter than these directions seem to make it.

The numerical results should always be stated in terms of the 1 sec. Thus, a reaction-time of 8 swings, or 8 fiftieths of a second, would be registered as .16.

LITERATURE. — The vernier was first applied to time measurement by the Leyden astronomer F. Kaiser (1806–1872), who explains the principle of application in two papers, published in 1851 and 1863.

The history of the 'personal equation' is extremely interesting, and should (if time permits) be given in lecture. A popular account will be found in Wundt's Human and Animal Psychology, 1896, Lect. xviii. For a more technical exposition, see Sanford, Amer. Journ. of Psych., ii., 1888–9, 3, 271, 403. The Instructor should also be familiar with Wundt's Phys. Psych., ii., 305–362, and with the article by L. Lange (Philos. Studien, iv., 1888, 479) in which the difference between 'sensorial' and

'muscular' reactions is noted and explained.[1] A recent paper by N. Alechsieff, in the Philos. Studien, xvi., 1900, 1 ff., contains a sound discussion (pp. 17–21) of the three forms of simple reaction.

It should, perhaps, be said, even in so elementary a treatment as the present, that there is some divergence of opinion as to the value and universality of the sensorial-muscular distinction. The reader who wishes to form a judgment in the question must acquaint himself with the literature of the reaction experiment (especially articles in Pflüger's Arch., Philos. Studien. Mind, Psych. Rev., Amer. Journ. of Psych.), and with the doctrine of mental types (fairly complete bibliography in L. W. Stern, Ueber Psych. d. individuellen Differenzen, 1900). The controversies seem to hinge on two different conceptions of the experiment. Some investigators regard the reaction method as a method for the discovery of psychological facts and laws in general; others regard the reaction experiment as an end in itself, as offering a certain typical consciousness (or, rather, a series of typical consciousnesses) for introspection. It is clear that the training required of *O*, and the directions given him before the experiment, will differ very considerably in these two cases. It is the second interpretation that is followed here.

A. (1) *The 'Natural' or 'Central' Reaction.*

PRELIMINARIES. — It is very important that *O*'s and *E*'s experimental records shall tally. Simple as the injunction is, that *everything* must be put down, it is an injunction which inexperienced students are continually disobeying. *E* and *O* should therefore prepare blank Tables, with lines numbered 1 to 10, and should have it impressed upon them that if there is nothing to enter, say, under 8, the next entry must nevertheless be made under 9. It is most annoying (and, without these precautions, it is very common) to have the records of 50 conscientiously performed experiments handed in by *E* and *O*, and to find a gap on the one side or the other which cannot be localised by memory, and which therefore prevents the intercomparison of times and introspections.

It is important, again, that *O* shall not centre his attention upon the length of the reaction experiment, and make it a point to 'react as quickly as possible.' If this tendency become mani-

[1] Lange, like most discoverers, was anticipated. The sensorial-muscular difference was remarked by S. Orschansky, in an article published in the Neurol. Centralblatt, 1887, no. 12, 265.

fest, he should be assured that he has nothing at all to do with the duration of the experiment: the time values are *E*'s affair, and come into account for *O* only in so far as they serve as a check upon his introspections. If *O* gives reaction times of .6 and .2 sec., and yet has no introspective difference to record as between the two experiments, it is clear that his introspection is inadequate: a .6 sec. consciousness cannot be identical with a .2 sec. consciousness. In this way, the times are useful; and they are useful, further, for comparison with the reaction norms. So many thousands of simple reaction times have been taken, and the times differ so little from observer to observer, and from year to year with the same observer, that we know approximately what the time in question 'ought' to be. But *O* is not concerned with the duration of reaction in any more direct way. He must not, by any means, try to estimate the time of a 'good' reaction, and then seek to reproduce this time in later experiments.

This second difficulty may be avoided, if *O* is free from prejudice at the outset, by a careful choice of words on the Instructor's part. The phrase 'as soon as' should never be employed. *O* is to move his finger 'on hearing' or 'when he hears' the sound; not 'as soon as possible after' he has heard it.

The Central or Natural Reaction. — It will be noticed that no specific directions are given in the text as to *O*'s manner of reacting in this experiment. He is left to face the situation in his own way, to react 'naturally.' Now we saw above, pp. xxv. ff., that the observers in psychological laboratories fall into two great classes, as subjective and objective. We shall, therefore, expect to find characteristic differences of reaction as between different *O*'s. "One man is accustomed to take up an active attitude to everything that occurs to him: his own action is for him the constant point of interest, and his environment is of importance only in so far as it affects this centre of reference; he therefore keeps his personality, his 'I,' in instant readiness for action. Another is wont to submit himself passively to the operation of external impressions; he regards them from the theoretical point of view, is contemplative in disposition. The former is inclined to make himself 'ready for the leap'; his

fingers are tense, and the psyche is intent; he merely awaits the signal for action. The latter tends as naturally to a sensorial direction of attention: if we force him to think of himself, and to dispose himself for the coming movement before the occasion for its performance has arisen, he feels confined and confused. The former is expecting his own outbreak, the latter is awaiting the impression; the stimulus is in the one case the release, in the other the cause of movement" (Stern, *op. cit.*, 108). The subjective observer, then, has a leaning towards the 'muscular' form of reaction; the objective tends of his own accord to the 'sensorial.' But as the conditions of the experiment require — or will seem to the unpractised *O* to require — a division of attention between expected stimulus and subsequent movement, the 'natural reaction times,' will probably, in every case, be greater than the practised muscular and less than the practised sensorial. Hence they have also received the name 'central.'

The types described by Stern are extreme types, between which lie many forms of mean. The purely subjective or objective *O* is, as we saw (p. xxvii.), rarely met with; most *O*'s are decidedly mixed in type. It is 'natural,' under these circumstances, that the central form should tend, as it does, somewhat definitely towards the muscular and away from the sensorial value. For *O*, unless he is unusually objective in type, can hardly avoid the self-suggestion, favoured by the very character of the experiment, that his movement is the most important feature in the case.

RESULTS. — It is unnecessary to give an illustrative 'natural' series. We may, however, quote in this place the various reaction norms.

	Sensorial.	Muscular.	Natural.
Sight	.27	.18	.19–.22 sec.
Sound	.23	.12	.14–.19 "
Touch	.21	.11	.12–.18 "

The numerical results may be treated in two ways. (*a*) The average of the series may be taken, and its *m. v.* calculated.

Let A be the average time, a, b, . . the separate times, and n the number of experiments. Then

$$m.\ v. = \frac{(A - a) + (A - b) + \ . \ . \ .}{n}$$

Practice is not complete until the $m.\ v.$ has fallen as low as one-tenth of the average time. (*b*) A 'curve of frequency' may be platted. The abscissæ are hundredths of a second; the ordinates vary in height with the number of times that the values of the corresponding abscissæ have occurred in the experimental series.

If the Instructor desires to follow the course of practice, the average and $m.\ v.$ of each part-series (10 experiments) should be taken. But it will be better to postpone this enquiry to vol. ii.

It is unnecessary, again, to give illustrative introspections. Unless O be of a pronounced 'type,' the attention will vary in direction for a little time, and then divide between stimulus and movement, tending rather (as we have seen) towards the movement. The composition of the motive will vary in the same way, until it settles down to a mixture of the sensorial and the motor, with the latter in the ascendant. The Instructor must assure himself, from series to series, that O is not letting the experiment become automatic, *i.e.*, decreasing the degree of attention. On the whole, however, the less the Instructor interferes with the course of the experiment, the better will it be.

The introspections will, naturally, be poor. O has had no practice in the observation of the action consciousness; and the present form of that consciousness, just by reason of the naturalness and obviousness of the mode of reaction, and the consequent vacillation of attention and complexity of motive, is peculiarly difficult of analysis. The introspections will improve in the next two experiments; and if the natural reaction be repeated at their conclusion, O will have a much greater mastery over his task.

One point must be clear from the first: the point that O is responsible for the results of the experiment. E is to average or plat the results. But O is to say whether this or that result shall be included in the average, or thrown out as a failure. He must decide, of course, without knowing anything of the actual times: neither E nor O should see his times until the whole

experiment, in all three forms and in all three sense-departments, is completed. *O* has a certain thing to do, — a certain consciousness to form, — in a series of tests; and he is the sole judge as to whether this thing has been done. If his attention has relaxed, if intruding ideas have forced themselves upon the attention, if there have been objective disturbances in the room around him, he must note the departure from the norm, and throw out the experiment. *E* is to sacrifice unquestioningly the results that he is told to sacrifice, no matter if (from the objective standpoint) they are 'perfectly good'; while, on the other hand, he is to let results stand that are obviously 'bad' results, if *O* declares that they are warranted by introspection.

It rests in the discretion of the Instructor, here as in the case of degree of attention, to interfere or not to interfere, if he sees *O* going astray. A good deal depends upon the character of the mistakes, and upon the time that can be allotted to the total experiment.

It need hardly be said that if *E* makes a mistake, — does not strike the key fairly, lengthens or curtails the signal period, — *he* must throw out the particular experiment. *O* then leaves a blank on his record sheet, and *E* proceeds to the next number in the experimental series. It need hardly be said, again, that *all* experiments, the rejected as well as the accepted, are to be entered in the note-book.

When the reaction experiment is performed upon a more elaborate plan, it is advisable: (*a*) to take a good number of practice series, the results of which are not counted; (*b*) to throw out the first two experiments of every series, no matter how 'good' they are, — on the principle that *O* must become adapted or 'warmed up' to his work after an intermission; (*c*) to reduce the introspective record to a set of simple symbols, so that the experiments may be taken in quick succession, and *O*'s general disposition may remain constant throughout a series; and (*d*) to extend the series to 20 experiments. All this is upon the assumption that the object of the work is the synthesis of the action consciousness, and not an investigation into the nature of practice, 'warming up,' variation of disposition, etc. — In the present case we have no time for such elaboration, and must therefore count in the results every time that *O* approves, from the beginning of the experiment.

(2) *The 'Complete' or 'Sensorial' Reaction.*

We now come to forms of reaction in which a definite direction of attention is prescribed. The instructions to *O*, in the sensorial reaction, will run somewhat as follows. "You are now to attend to the sense impression, the sound. When the 'Now!' comes,

you are to think of, and look out for, the sound. Let the movement of your finger take care of itself; it will follow all right, when you have heard the sound. And be sure that you think of *the* sound; get it impressed on your memory, so that you would not be tempted to react to any other kind of sound." The Instructor can afford to slur the movement in this way, since its association to the stimulus has already become ingrained by the previous experiments. He must be on his guard that there is no misunderstanding as to the mode in which the sound is to be identified: *O* is not to wait, and think about it, and ask himself if it is the right sound, but to be so thoroughly prepared for it that he identifies it by direct apprehension.

Different *O*'s will carry out these instructions in different ways, according to their 'idea types.' One man will ideate or image the sound itself; another will keep a verbal description of it in the forefront of consciousness; another will see the rod striking the button of the key, etc., etc. The most constant factor is the group of strain-sensations characteristic of adaptation of the sense-organ (p. 209 above). See Külpe, Outlines, 408; Wundt, Phys. Psych., ii., 316, note.

E's work is as before, except that he is to introduce an occasional 'puzzle stimulus' into the course of a regular series. He may, *e.g.*, strike the table instead of the key with his wooden rod, or strike the key with the handle of his pocket-knife. If *O* is rightly disposed, he will make no movement in response to these puzzle stimuli. If he reacts, it is because there is still a 'muscular' ingredient in his preparation for the experiment: further practice is then needed.

Question (4) See Primer of Psych., 179 f.; Outline, 341. The Instructor must be careful (as one of the author's students said) not "to make the motive too conscious"; *i.e.*, not to exaggerate the clearness and discriminability of its constituent ideas. When once *O* has laid hold of the motive, the task of introspection is not especially difficult; but the preceding verbal analysis is apt to suggest (as another student put it) that "the ideas are a good deal more dignified than you actually find them to be." It is true, as Stumpf says (Tonps., i., 162), that "vermittelnde Vorstellungen können weniger lebhaft und doch unentbehrlich sein."

RESULTS. — The following are typical series.

(1) First series taken after the 50 'central' reactions.

Exp.	Time.	Introspections.
1	.32 sec.	Tried to attend to the stimulus, but had a motor image of my own movement. The stimulus came later, and was less decided, than I had expected. This disturbed me. — Bad.
2	.34 "	Attention entirely on the stimulus. Had a visual image of my movement after reacting. — Good.
3	.32 "	Attention distracted a little by a picture that *E* had suggested to me just before the experiment. Visual image of my movement; but consciousness was filled with the idea that I must move and move quickly. — Bad.
4	.22 "	Attention wholly upon stimulus. Visual image of my own movement, coincident with the pressure of actual movement. — Good.
5	.14 "	Attention on stimulus. Visualised my own movement, as before. — Good.
6	.12 "	Tried to attend to stimulus, but had a sense of being forced to move, and to move quickly. As I moved, thought that I pressed harder than was necessary to release the pendulum. — Bad.
7	.24 "	Seemed to be getting more familiar with the experiment and to realise what the three ideas [motive ideas] meant. Attention on stimulus. — Good.
8	.23 "	As before. — Good.
9	.30 "	Attention on stimulus, but with some innervation of hand and arm. — Think it was good.
10	.20 "	Attention on stimulus. No picture of movement till I felt the pressure of moving; then I found myself trying to introspect. — Good.

In almost every case, there was a visualisation of the pendulums after the movement of reaction had been made. In a few cases there were shadowy ideas of result. References to adaptation of the sense-organ have been omitted.

Taking the 'good' times of the above list, we have an average of .238 sec., with an *m. v.* of .047 sec. This *m. v.* is a great deal too high. Looking at the introspections, we find that a consciousness which lasts .34 sec. is equated with a consciousness which lasts but .14 sec.; it is clear that introspection is not yet adequate to its work. On the other hand, *O* is capable and

honest as regards his 'muscular' tendencies, and shows marked improvement as the series progresses.

(2) Fifth series taken.

Exp.	Time.	Introspections.
1	.26 sec.	Visual idea of apparatus, while waiting for stimulus. Idea of result: a little anxious to make a good series. Strain in finger just as it pressed the key; none before. — Good.
2	.22 "	Auditory idea of stimulus; idea of result visual, experiment done; mood familiar and of-course; no effort or strain. — Good.
3	.20 "	Idea of sound as before; idea of result just comfortableness and of-course mood. — Good.
4		False stimulus; no tendency to movement, and no mental disturbance.
5	.26 "	Idea as before. Perhaps a little hesitancy in getting the sound, owing to false stimulus: of-course changed to careful. Otherwise good.
6	.20 "	Sound clearly imaged; mood of-course; very smooth reaction. — Good.
7	.20 "	Feeling of non-responsibility and certainty. Idea clear. — Good.
8	.24 "	A little fuller; idea of result back again. Otherwise as before. — Good.
9	.30 "	Everything seemed very clear; this was the best experiment so far for introspection. — Good.
10	.20 "	As 7. — Good.
11	.22 "	Same. — Good.

The average here is .23 sec.; the *m. v.*, .028 sec. It is the merest accident that the average time equals the norm. Despite the result of exp. 4, *O* has not yet entirely overcome his muscular tendency: this is proved partly by the reference to 'smoothness' and 'certainty' in the introspections, the 'smoothness' implying some degree of motor preparation; and partly by his later work. — References to adaptation of the sense-organ are omitted.

(3) *The 'Abbreviated' or 'Muscular' Reaction.*

The instructions to *O*, in the muscular reaction, will run as follows. "You are, in this series, to attend to the latter part of the experiment, your own movement. You are to prepare your-

self for movement, just as in the last series you prepared yourself for the 'direct apprehension' of a sense-impression. When the 'Now!' comes, you are to be on the alert to move. In the last series, you associated a movement to the sense-impression; in this series, the sense-impression gives you the opportunity to make a movement that you have been wanting to make ever since you heard the 'Now!'"

Different *O*'s will, again, carry out these instructions in different ways. Müller (in Pilzecker, *op. cit.*, 65) speaks of a *Bewegungsbild*, a motor image, which is of the same character as the stimulus image in the sensorial form. Wundt (*loc. cit.*) finds no motor image in the reaction consciousness, but emphasises the strain-sensations in the reacting muscles: if he has an image, it is a faint (presumably visual) image of the reacting member. The differences will appear in the introspections.

Characteristic of the muscular mode of reaction is the occurrence of premature and of erroneous reactions. The reaction is termed premature when *O* moves his finger before the stimulus has been given by *E*; it is termed erroneous, when he reacts not to the tap of the key but to some other accidental stimulus. On the subjective side, we find that *O* not infrequently regards the stimulus and the answering movement as simultaneous.

On the composition of the motive, see Primer, 180; Outline, 343.

RESULTS. — The following series (second taken) is typical.

Exp.	Time.	Introspections.
1	.12 sec.	Strain in finger; attention on finger. Stimulus came to consciousness when I reacted or even a little later.
2	.12 "	Finger ready; attention on it. Time between 'Now!' and pressure seemed long. Relief to move.
3	.12 "	Attention all on finger. Felt strain go along arm from elbow to finger.
4	.12 "	Attention on finger; but sense of strain less prominent. Effort to direct strain.
5	.14 "	Strain in finger, but attention not strong.
6	—	Reacted to the 'Now!' without knowing what I was doing.
7	.14 "	Strain in finger, but not much attention to direct the effort.
8	.12 "	Better: strain again from elbow to finger.
9	.10 "	Whole thing very easy; not much strain in finger.
10	.08 "	As last experiment.
11	.14 "	More attention; but strain in finger not prominent.

All the experiments were 'good.' The average time here is .12 sec., and the *m.v.*, .012 sec. The fatigue of the first 8 experiments has tempted *O* into a lapse of attention; experiments 9 and 10 are getting towards the reflex. *O* recovers himself again in 11.

These and the foregoing results are *typical*, not excellent. In some cases the introspections are fuller and more accurate; in others they are more scanty. The three series represent the average work that can be accomplished in the time allowed.

B. We have begun with the description of reactions to sound, because the vernier chronoscope lends itself most easily to sound work. *E* will, doubtless, require some little practice before he can strike the button of the stimulus key with approximately equal force throughout an experimental series; but the training needed is minimal. Pressure reactions, on the other hand, introduce a complication on the side of *O*, and visual reactions a complication on the side of *E*. Nevertheless, there is no intrinsic reason why all students should begin with sound. If several pairs are occupied with reaction work at the same time, it will be better to distribute the experiments evenly over the three sense-departments. All the general directions given above in regard to sound apply with equal force to touch and vision.

PRELIMINARIES. — *E* must practice the 'flick' of the finger that releases the key. It is not difficult to get the knack of a pressure that shall be clean-cut and yet not intensive.

Sanford (196) recommends a different procedure. "Reactions to touch, or more exactly to pressure, may be tried by having the subject place one forefinger under that of the operator on the operator's key. He will thus receive a pressure in it at the instant that the operator's pendulum is released, and can release his own pendulum with the other finger." The author has found this arrangement less satisfactory than that of the text, especially in the case of muscular reactions.

If cutaneous reactions are the first to be tried, 300 experiments should be taken, as recommended above for sound.

C. PRELIMINARIES. — A diagram of the arrangement for deadening the noise of the key is given in Primer of Psych., 185. The screen must stand in a good light; and the stimulus paper in the clip must lie as close as possible to the screen surface.

Visual reactions may be taken, without the screen and side-wire, as follows. At the "Now!" *O* fixates *E*'s finger, which lies upon the stimulus key. The movement of *E*'s finger is the visual stimulus to reaction. — These experiments are, however, much less satisfactory than the others.

RELATED EXPERIMENTS. — The simple reaction may be varied by changing, *e.g.*, the quality or intensity of the stimulus. Thus it is easy to attach a small bell-gong to the stimulus key, and to release the pendulum by the stroke which sounds it. In visual reactions, different colours may be used, as well as black and white. The touch reactions may be modified, similarly, by substituting a cooled or heated cylinder for the rap of *E*'s finger. As for intensity: two forces of stroke may be employed, whether with the wooden rod or with the gong. In visual reactions, a grey card of the same brightness as the screen may be put in the clip, and the position of the opening changed, so that it is exposed and not filled by the movement of the stimulus key. Then a flash of sunlight or electric light, reflected through the opening from a plane mirror, may serve as stimulus. For touch, two intensities of pressure, or of heat and cold, can be applied. In all these cases, it is the central form of reaction which is most interesting. Very weak stimuli, *e.g.*, evoke a sensorial reaction, even in the absence of all directions to *O*. Very intensive sounds may, according to circumstances, call forth a short or a long reaction: short, if *O* is muscularly disposed; long, if he tends towards the sensorial type. In the former case, the reaction is carried along on the current of the intensive stimulus; in the latter, the attention is arrested, shocked, by the loudness, and the movement is for a moment inhibited. — Wundt, ii., 344 ff.

Still more interesting is the course of the experimental series when the ready-signal is sometimes given and sometimes omitted. In such a series the word "Ready!" tells *O* that he is to lay his finger on the key, and the word "Now!" given a few seconds later is (as always) the ready-signal proper, the call for attention. This "Now!" is put in and left out, irregularly. At the end of the series, the results and introspections 'with signal' are separated from those 'without signal,' and the two part-series compared. All three forms of reaction, natural, complete and abbreviated, should be tried. — Wundt, 348 f.

Thirdly, a series may be taken with irregular alternation of weak and loud sounds; Wundt, 351. The natural form of reaction should be adopted.

Fourthly, reactions may be taken, with a constant intensity of stimulus (auditory, visual or tactual), but with a concomitant 'distracting' stimulus; *e.g.*, the whir of the kymograph clock. All three forms may be employed. Wundt, 353 f.— The results of these additional experiments are of importance for a general theory of action. But it is essential that the work outlined in the text be thoroughly performed, before they are attempted.

Question (5) The norms have been given above. The average difference is 0.1 sec. For the explanation, see Külpe, 407 f.; Pilzecker, 65 f.; Alechsieff, 19; Wundt, ii., 309, 315 f.; G. Martius, Philos. Studien, vi., 1891, 191 ff.; L. Lange, *ibid.*, iv., 1888, 497 ff.; Titchener, Mind, N. S., i., 1892, 220.

Against the sensorial-muscular difference, see especially J. M. Baldwin and W. J. Shaw, Psych. Rev., ii., 1895, 259; J. M. Baldwin, Mind, N. S., v., 1896, 81; J. McK. Cattell, Philos. Studien, viii., 1893, 403; J. McK. Cattell and C. S. Dolley, Memoirs of the Nat. Acad. of Sciences, vii., 1896, 409 f.; Psych. Rev., i., 1894, 165; T. Flournoy, Arch. des sci. phy. et nat., xxvii., 1892, 575; xxviii., 1892, 319; Observations sur quelques types de réaction simple, Geneva, 1896; J. R. Angell and A. W. Moore, Psych. Rev., iii., 1896, 245 (Univ. of Chicago Contr. to Philos., i., 1); J. R. Angell, *ibid.*, v. 1898, 179 (Contr., ii., 2, 179). *Cf.* Titchener, Mind, N. S., iv., 1895, 74, 506; v., 1896, 236; Stern, *op. cit.*, 103.

(6) Smell experiments can be made as follows. Connect a glass thistle-tube by rubber tubing to a small rubber bulb which contains a few drops of oil of cloves. Pack a little loose cotton wool at the bottom of the thistle-cup. At the "Ready!" *O* places his finger on the key, and takes the thistle-cup in his left hand. At the "Now!" he holds the cup to his nose. See that the "Now!" falls at the beginning of an expiration. Lay the bulb on the bottom of the stimulus key, and, as inspiration begins, press bulb and key, thus releasing the long pendulum.

For taste experiments, *E* may use the rubber syringes of Exp. XXIII. The squeeze of the bulb and the pressure of the key must be made as nearly simultaneous as possible.

Neither of these procedures is at all exact; but taste and smell reactions are never very satisfactory. Wundt, 317 f.

It remains to suggest a method for taking reactions to *pain*. A stout flat spring is nailed at one end to a cap of wood, which fits over the button of the stimulus key. At the other, free end of the spring, a pin or small brad is driven through the metal. *O*'s left hand is so placed that the pressure on the stimulus key which releases the long pendulum, brings the pin or brad point sharply down upon the skin of the palm.

(7) Within the limits of the theory of action, we have the related experiments cited above, and the whole series of compound reactions: Wundt, 362 ff. If we regard the reaction method simply as a psychophysical method for the study of mind at large, we have to mention (*a*) the association reaction (Outline, 352 ff.); (*b*) the fusion reaction (351); (*c*) the intensity-quality reaction (351). The method is also of value (*d*) for the investigation of the course of practice, habituation, expectation, fatigue. Finally, travelling outside of psychology, we must note that physiology has been interested in the simple reaction, as a means for the determination of the velocity of nervous impulses (*cf.*, however, Cattell and Dolley, Mem. Nat. Acad. of Sci., vii., 1896, 393 ff.); and that astronomy is similarly interested, seeing that a reaction is involved in the observation of stellar transits (Alechsieff).

(8) This question must be answered from the introspections. It will not be amiss, when the whole experiment is completed, to let each student enter in his note-book (*a*) the average time and *m.v.* of all the *O*'s in the class, and (*b*) the corresponding analyses of motive. The striking uniformity of time values, and the equally striking divergence of introspective results, are most instructive. The former represent the reaction as a fundamental function of mind, practically identical in all *O*'s; the latter show how various the processes may be that carry a given function.

(9) Outline, 350 f. *Cf.* Bentley, Amer. Journ. of Psych., xi., 1900, 412.

(10) Primer, 181 f., 262.

(11) The practised consciousness is characterised on the functional side by (*a*) maximal degree and constant direction of attention, with minimal effort (secondary passive attention);

(*b*) ease of discrimination (delicacy of perception); and (*c*) high degree of capacity of reproduction, implying great readiness and confidence of judgment. — Külpe, Outlines, 42 f., 212, 302 f. On the physiological basis of practice, see Wundt, i., 236, 279; ii., 473.

It is well to remind ourselves, in these days of lightless and soundless reaction-rooms, that one of the most direct effects of practice is a narrowing of attention to the practised subject-matter. Practice makes us 'mentally deaf' and 'mentally blind' to distracting stimuli, much more effectively than does the mechanical removal of the grosser distractions. *Cf.* also the remarks on distraction, p. 218 above.

INSTRUMENTS. — Another good and cheap instrument is H. Obersteiner's Psychodometer: L'Année psych., v., 1899, 394. *Cf.* James, Psych., i., 1890, 87.

PART II

PERCEPTION IDEA AND THE ASSOCIATION OF IDEAS

CHAPTER IX

Visual Space Perception

§ 45. **Perception.** — The sketch of the psychology of perception given in the text differs in many points from the treatment found in the psychologies. Perception is ordinarily regarded as the simplest cognitive function of the 'real' mind: "we cannot perceive without perceiving something" (Stout, Manual of Psychology, 241). But this is a question, partly of terms, and partly of standpoint. Would it not be as true (or as untrue) to say: we cannot sense without sensing something? It all depends (1) upon what you mean by 'sensing' and 'perceiving,' and (2) upon the point of view from which you wish to consider sensation and perception.

We are concerned, in the laboratory, not with the 'objective reference' of perception, not with perception as meaning 'the direct cognition of present objects,' but with the perception viewed as mental stuff, arranged by nature, modified in the course of natural evolution.[1] What sense-material have we in it? Under what general type of connection may the plan or

[1] It is a pity that we have no words but 'perception' and 'percept,' words surcharged with functional meaning, for laboratory use. If we borrow from geometry the term 'connex' ("any mixed form consisting partly of points and partly of lines, or of other diverse elements": Century Dict.), we might speak of 'sense connexes' in place of 'perceptions.'

pattern of this material be classified? What substitutions, reductions, transpositions, has the material undergone?

It would seem, at first sight, that the easiest perceptions to begin with are those technically known as 'qualitative perceptions': complexes like lemonade, or a chord in music. In them, sensations are put together as qualities, not as intensities or extents or durations, so that qualitative analysis is, so to speak, upon its own ground. Moreover, such perceptions are far less liable to change than spatial and temporal perceptions are. Quality is the absolute, permanent attribute of sensation: intensity and extent and duration are relative, constantly changing. Lemonade and the common chord are the same for me, the same conscious stuff, as they are for a child of two; but my spatial and temporal perceptions are different from the child's.

Nevertheless, it is better, in the present state of psychology, to begin elsewhere. One qualitative perception, the tonal fusion, has received thorough investigation in Stumpf's Tonpsychologie (vol. ii., 1890). Unfortunately, however, the psychology of work in this subject demands a good deal of preliminary physical knowledge, and accurate and expensive instruments. The other qualitative perceptions, smell and taste fusions and organic complexes, are as yet hardly known at all in detail. So that, while theoretically the qualitative perception is the easiest to start with, it is not the easiest to begin upon in laboratory practice.

For the rest, the psychology of perception, in any form, is a good deal more difficult for *O* than the psychology of sensation, and would be more difficult for *E*, also, were we to leave the beaten track of experimentation. Illustrations are not far to seek. Although ten years have elapsed since the publication of Stumpf's monograph, in which, as we have said, 'tonal fusion has received thorough investigation,' yet there is still wide difference of opinion as to his results, as to choice of methods, and as to the relative value for research of musical and unmusical persons, while there is scarcely a suggestion of a true 'theory' of the tonal fusion, *i.e.*, a definite statement of the proximate conditions under which it is realised. Space theories have ranged between almost polar opposites; and we are only

now beginning to have a theory of the time perception. All this means that the task set to introspection is immensely difficult; and actual investigation soon reveals the fact that the difficulty is twofold, intrinsic and genetic. Sensations that our physical environment has joined together are hard to put asunder; and sensations that were joined together in times when the organism was still plastic are hard even to recognise.

Let us look at some instances.

(1) Wundt's 'genetic' theory of space perception holds that "the spatial order is developed from the connection of certain sensation components, which taken separately have no spatial attributes whatsoever" (Outlines, 127; Grundriss, 150). Visual space perception, *e.g.*, is the resultant of retinal sensations multiplied into (if we may use the phrase) the sensations set up by eye movements. "The attributes of mental complexes are never limited to those of the mental elements that enter into them; new attributes, peculiar to the complexes as such, always arise as the result of the connection of the elements. Thus, a visual idea has not only the attributes of the visual sensations, and of the sensations from ocular position and movement, which are contained in it, but also that of the spatial arrangement of the sensations, which these elements as such do not possess" (Outlines, 91; Grundriss, 107). The equation, in principle,—narrowed down for simplicity's sake,—is:

$$\begin{array}{c}\text{Visual sensation (varies}\\ \text{in intensity and quality)}\end{array} \times \begin{array}{c}\text{Articular sensation}\\ \text{(varies in intensity)}\end{array} = \text{Space.}$$

In criticism of this theory, we have the following to say. (*a*) When we dissect the adult space consciousness, we find that the attribute of extent, spread-outness, cannot be divorced from the visual and cutaneous sensations. Extent, the logically simplest spatial determination, is given directly with a blue or a pressure. We cannot, by analysis, go behind this given fact. (*b*) But the logically simplest is not necessarily the earliest in genesis. An amorphous tissue, in the animal body, may be primitive, but it may also be the final product of a long series of structural changes. Hence we may not argue that because extent is now a sensation attribute, it has been a given charac-

teristic of mental process from the very beginning. We must keep our analysis and our genesis distinct. (*c*) Having entered this *caveat*, we are bound (so it seems to the author) to accept the equation. We must take our two sensation series, and try to find out precisely under what conditions of connection the space perception arises, and how it varies with variation of these conditions. This is the second part of our space problem, as analysis is the first. (*d*) We must not, however, put an immediate genetic interpretation upon an equation whose terms are analytical. The 'visual' sensations and 'articular' sensations whose fusion is space are not *our* visual and *our* articular sensations, but their primitive equivalents or representatives. The reconstruction of these is the third part of the space problem.

(2) Stumpf prefaces his theory of tonal fusion by a consideration of all the possible psychological conditions. Fusion may be the expression of some general law of the interaction of ideas; it may depend upon the similarity of the fusing sensations; it may be due to the mixture of concomitant feelings, or to the degree of smoothness (relative freedom from beats) of the component sensations; it may result from the frequency of their juxtaposition in consciousness (Tonpsychologie, ii., 184–211). All these possibilities are discussed, and found wanting. Hence the condition of fusion must be physiological; and a further examination of possibilities leads to the idea of *specific synergies* of the cerebral cortex. A specific synergy is a "determinate mode of coöperation of two nervous structures, having its ground in the structure of the brain, of such a kind that whenever the two structures give rise to their corresponding sensations there arises at the same time a determinate degree of fusion of these sensations" (p. 214; *cf.* Beitr., i., 50 ff.).

Not much more can be said of this synergy. But though we are to prefer "honest poverty to suspicious wealth," the question of tonal fusion cannot be left here. Stumpf is impelled to write a further paragraph upon the "generic development of fusions." The physiological mechanism of synergy, connate with the individual, may have been acquired in the course of generations, and perhaps the influences under which it took shape may have been in part mental influences. Impressions

that affect the sense-organ together, with relative frequency, might gradually set up a cortical disposition to conscious fusion; and our ancestors might be led by pleasure, by some purpose, by one mental motive or another, to subject themselves to impressions of this natural frequency of coincidence. The fact of fusion, as we know it now, would thus depend indirectly upon mental influences (p. 215). Stumpf even proceeds to make his ideas concrete, picturing, *e.g.*, primitive man and woman as uttering signal-calls at different pitches (pp. 215 ff.; cf. the parallel thought, as regards the origin of our feeling for the purity of musical intervals, in the Zeits. f. Psych., xviii., 394).

Stumpf himself calls these suggestions "vague conjectures," hypotheses that contain "too many part-hypotheses which we have at present no means of checking," rather speculations than even hypotheses (p. 218). But the important point is that, though they are vague suggestions, and though we have no means of testing them, the writer still feels it necessary to venture upon them; his psychology of fusion would not be complete if they were omitted. The instance shows with great clearness how essential the third part of our perception problem is to the problem of perception at large.

§ 46. **Visual Space Perception: Preliminary Exercises.** — It is hardly necessary to say that the Questions and Exercises of this Section may be extended by the Instructor to far greater length. The author has given merely the essentials of the subject: things that must be known, and the knowing of which implies a general familiarity with the structure and function of the eye as a 'space organ.'

(1) *The Reduced Eye.* — The first complete reduction is given by J. B. Listing (1808–1882), in the article Dioptrik des Auges (Mathematische Discussion des Ganges der Lichtstrahlen im Auge), printed in Wagner's Handwörterbuch der Physiologie, iv., 1853, 451 ff. (esp. 485–496). Listing proceeds to the 'reduced' by way of the 'schematic' eye, a system with six cardinal points, figured on p. 492. The student should consult this article, if it is available.

Consult also: Helmholtz, Physiologische Optik, 2d edn., 5, 85 ff. (esp. 89 f.), 140; Aubert, Physiol. Optik, 441 f.; Fick, in

Hermann's Handbuch der Physiologie, iii., 1, 61 ff.; Foster, A Text-book of Physiology, iv., 1891, 1144–1148; Waller, An Introduction to Human Physiology, 411 f., 424.

QUESTIONS. — The Questions of this Section are not based upon the statements of the text; they are rather questions, the answers to which must be presupposed if the text is to be fully understood. The student should read the text, getting what he can from it, — the amount will vary with his knowledge of physiology, — then proceed to the Questions, and then reread the text in the light of his answers. The Instructor can assist very largely by means of lectures and prepared diagrams.

(1) Remember that the first Table must include distances along the optical axis of the system, and the second the radius of curvature of the ideal surface. The values should be given accurately, not in round numbers as in the text.

(2) 'Optic axis' here means the optical axis of the eye: the straight line drawn through the centres of curvature of cornea and lens, and prolonged to meet the retina between the yellow spot and the place of entry of the optic nerve.

(2) *The Formation of the Retinal Image.* — There may be students who are troubled by the inversion of the retinal image. The difficulty is imaginary, and should be somewhat summarily dismissed. *Cf.* the author's Outline of Psychology, 177 f.; Wundt, Human and Animal Psychology, Lect. x., § 5; Foster, Physiology, iv., 1216 f.

On the retinal image in general consult: Helmholtz, Physiol. Optik, 85 ff., 109 f.; Wundt, Physiol. Psych., 4th ed., ii., 98 f., 106 f.; Foster, Physiology, 1149 f.; Waller, Human Physiology, 413 f., 419 f.

QUESTIONS. — (3), (4). See references above.

(5) Optic axis, Helmholtz, 90 f., 108 f.; line of vision, Wundt, 99; of regard, 99; principal sighting line, 106.

(6) Helmholtz gives the following methods. (*a*) Cut away the sclerotic and choroid coats of a freshly removed eye, leaving the retina intact. Look through the eye, from behind, at some bright object. The image is "small, bright, distinct, and inverted." (*b*) Gerling's method. Remove the retinal elements with a fine brush, and insert a glass or mica plate in the opening.

(*c*) Remove the eye of a white rabbit, and observe as in (*a*). [This is the most available method: it has been worked out by Sanford (Lab. Course, exp. 104, p. 89). The outer coats need not be cut away. The eye is mounted in a clay ring, for convenience of handling. Sanford remarks that images of distant objects are clearer than those of near objects: the dead eye is adjusted for distant vision. This is a matter of accommodation: *cf.* Helmholtz, Physiol. Optik, 144. He recommends, further, that convex and concave spectacle-glasses be brought before the rabbit's eye and the eye of the observer, and the similarity of effect in the two cases noted.] (*d*) Volkmann's method. Select as subject a blonde, blue-eyed individual, with somewhat projecting eyes. Seat him in the dark room. Let him turn his eyes as far as possible to the right, so that the cornea of the right eye occupies the outer angle of the eyelids. The normal eye turns readily outward through an angle of 40°; Helmholtz found that he could turn with effort through an angle of 50° (Physiol. Optik, 615; *cf.* Hering, in Hermann's Hdbch., iii., 1, 442 f.). Now hold a lighted candle (or set some other source of light) on the subject's right, at an angle of 80° to 85°. Standing before him, you see the inverted retinal image of the flame, through the investing coats of the eyeball, near the inner angle of the right eyelids. (*e*) Ophthalmoscopic observation of the retina. — Physiol. Optik, 85 f.

(7) When we take into account the range, quickness and accuracy of eye movements, we must consider it an advantage, for 'concentration of attention,' that all parts of the field of vision are not seen by the resting eye with equal distinctness. *Cf.* Wundt's psychophysical law of the 'correspondence of apperception and fixation,' Phys. Psych., ii., 121 f.

(3) *The Mechanism of Accommodation.* — For detailed treatment of this question, see Helmholtz, Physiol. Optik, 112–156; Foster, Physiology, 1151–1158, 1168–1184.

Questions. — (8) See Helmholtz, 134; Foster, 1170.

(9) The images of reflection on the anterior and posterior surfaces of the lens were discovered by the Austrian physiologist J. E. Purkinje (1787–1869), and described in his tract

De examine physiologico organi visus et systematis cutanei, 1823. They were employed for diagnostic purposes by the French surgeon L. J. Sanson (1790–1841; Leçons sur les maladies des yeux, 1837), and are now generally known by his name.

The essentials of the experiment are as follows. The subject sits in a dark room, from which all light but that of the experimenter's candle is excluded. One eye is closed by a bandage. A far and a near fixation-point are marked (say, at 30 and at 300 cm.) along the line of vision of the open eye. In front of this eye, to one side and upon the same level, is placed the candle flame; the experimenter, who is observing the eye, sits on the other side of the subject, in such a position that the angle *ESf* is approximately equal to the angle *CSf*. After a little shifting of the eyes to and fro about the point *E*, the experimenter will

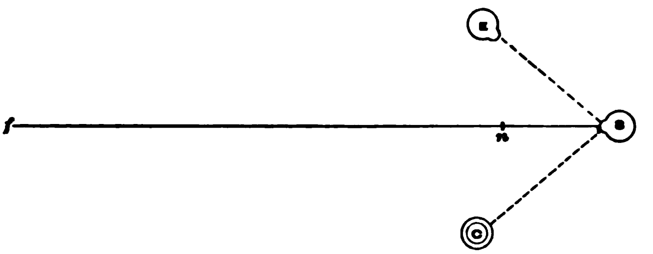


FIG. 45. — Helmholtz, Phys. Optik, 1896, 132. *E*, experimenter; *S*, subject; *C*, candle; *n*, near; *f*, far.

see three reflected images of the flame in the eye *S*. The first (nearest the light) is very bright; it is an erect, virtual image from the convex surface of the cornea. The middle image is that reflected from the less convex anterior surface of the lens: an erect, virtual image, larger than the corneal, and apparently situated some 8 to 12 mm. behind the centre of the pupil. This image is very indistinct ("more like a light cloud than an image," as Sanford remarks: Lab. Course, 94), and owing to its position is easily lost with any shift of *C* or *E*. The third image, lying on the nearer edge of the pupil, at an apparent depth of 1 mm., is reflected from the concave posterior surface

of the lens; it is a real, inverted image, visible as a small spot of brightish light.

When the subject accommodates for the near point, the middle image becomes smaller, and approaches the corneal image; when he accommodates for the far point, it grows larger again, and retires from the corneal image. No change at all is observable in the corneal image itself; and none, or hardly any, in the image from the posterior surface of the lens. It follows (from the laws of reflection in convex mirrors: Helmholtz, 133) that the essential thing in accommodation is the change in convexity of the anterior surface.—

FIG. 46.—Sanson's images (Helmholtz, 132). *a*, corneal image; *b*, image of anterior lens-surface; *c*, image of posterior lens-surface.

This is the experiment in the rough. The observations are easier to make, and more striking, if two images are reflected from each surface; since in that case the two middle images approach each other at the same time that they grow smaller. We may employ for this purpose a vertical screen with two windows, behind each of which stands a lamp or candle (Helmholtz, 132); or a single flame and a horizontal mirror (*ibid.*); or Helmholtz' phacoscope (Gk. φακός, lentil, lens, and σκοπεῖν, to view). The arrangement of the experiment should be left, as far as possible, to the ingenuity of the students. Good diagrams will be found in Aubert, Physiol. Optik, 444; *cf.* Sanford, 94.

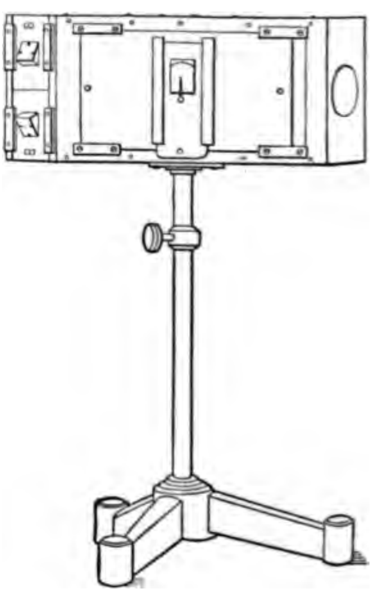


FIG. 47.—Phacoscope.

(10) Scheiner's experiment is so named from the Jesuit Rector C. Scheiner (1579–1650), who published in 1619 his treatise:

Oculus sive fundamentum opticum, in quo radius visualis eruitur, sive visionis in oculo sedes cernitur et anguli visorii ingenium reperitur. It is interesting for three reasons. It brings the mechanism of accommodation into direct comparison with the action of the lenses and screens that we are familiar with in optical instruments; it illustrates the laws of double images seen by a single eye; and it emphasises in an instructive way the fact of the inversion of the retinal image. It is performed as follows.

Prick in a card two fine, smooth holes, 1 mm. (or any distance less than the diameter of the pupil) apart. Mount two white pins on corks.

O sits with his back to a window; one eye is bandaged. A black cloth screen stands on the table before him, some 75 cm. away. The card is held up close to the open eye, the pin-holes horizontal. The white pins are set up along the line of vision, at distances of 20 and 50 cm. respectively. If, now, he looks at either pin, it is seen single and sharply outlined. But if he looks at the nearer pin, the farther pin is indistinct and double; and if he looks at the farther, the nearer is indistinct and double. Moreover: if one of the pin-holes be covered (by another card, or by a finger) while either pin is being fixated, there is no change in the image of that pin: the whole field is simply made somewhat darker. There is a change, however, in the double images of the pin which is not fixated. If *O* looks at the nearer pin, and the left pin-hole is covered, the left-hand single image of the farther pin disappears. The double images are same-sided, or 'uncrossed.' If, on the other hand, he looks at the farther pin, and the left pin-hole is covered, the right-hand single image of the nearer pin disappears. The double images in this case are different-sided, or 'crossed.'

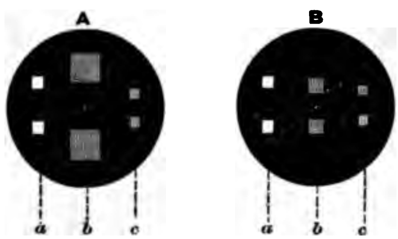


FIG. 48. — Phacoscopic images. A, far fixation; B, near fixation. *a*, *b*, *c*, as in Fig. 46. Helmholtz, 133.

The same experiment may be tried, in diffuse light, with a

white screen and black pins (small shawl pins); with the pins placed horizontally, and the pin-holes vertical; and with three pin-holes instead of two. The shifting of the three images, with differences of arrangement of the three pin-holes, should be recorded and explained.

The results of the experiment are explained by the accompanying diagram. A biconvex lens *b* receives from the luminous point *a* two pencils of rays which pass through the openings *e*, *f* of an opaque screen. The rays come together at the point *c* of the screen *nn*. We may, now, regard the lens *b* as the refractive medium of the reduced eye, the anterior screen *ef* as the card held before the pupil, and the posterior screen *nn* as the retina. The diagram will then represent Scheiner's experiment

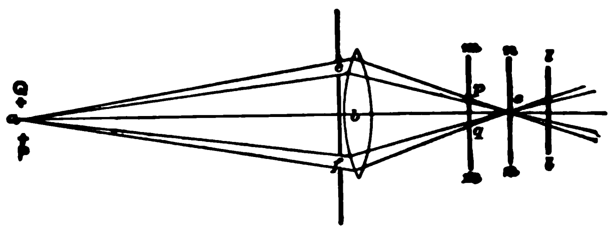


FIG. 49. — Helmholtz, 117.

with the eye either in horizontal or in vertical section. In the former case, *a* is a transverse section of the vertical pin, and the holes *e*, *f* are horizontal; in the latter, *a* is a transverse section of the horizontal pin, and the holes *e*, *f* are vertical. If the eye is accommodated for *a*, the needle is imaged clearly and singly at *c*. If it is accommodated for a point beyond *a*, the retina (so to speak) moves forward to some such plane as *mm*. We have two dispersion images of *a*. If, finally, it is accommodated for a nearer point, the retina moves back to *ll*; and we have again two dispersion images.

There is, however, an apparent discrepancy. If we cover the hole *e*, and the retina is at *mm* (far fixation), we cut off in the diagram the pencil *p*, whereas we cut off in the experiment the opposite single image. Conversely, if the retina is at *ll*

(near fixation), we cut off in the diagram the pencil q, whereas we cut off in the experiment the same-sided single image. The contradiction disappears when we remember that the retinal image is inverted. An image p (above or to the right) must come from a point in objective space P, below or to the left; and so with q and Q. — See Helmholtz, 117. Good diagrams are in Waller, 421 f.; Sanford, 91.

(11) The following errors may be noted:

(*a*) *Faulty Centering.* — The lenses of the instrument may be of good quality, but may be badly centred, so that the refracting surfaces do not lie truly about the optical axis of the system.

Observation of Sanson's images shows that the eye has this defect: Helmholtz, 108 f.; Fick, 60. The proof is, theoretically, very simple, and may well occur to a student who has been interested in Question (9) above.

(*b*) *Chromatic Aberration.* — In good optical instruments, the lenses are 'achromatic' or 'apochromatic,' *i.e.*, have been corrected for chromatic aberration. This is not the case with the eye. The violet focus lies nearer to the lens than the red.

There are three chief reasons why we do not notice chromatic aberration in ordinary vision. The refracting medium of the eye is water [see above, under (1)], not glass: in the light by which the eye is ordinarily affected, the red and violet (least and most refrangible) rays are intrinsically weak, while at the same time their light is distributed over larger dispersion circles; and the iris cuts off peripheral rays. To these may be added, in the case of those who wear glasses, the long focal distance of spectacle lenses. We can, however, easily assure ourselves of the existence of this defect in the eye. — Helmholtz, 157, 164; Fick, 101; Suter, Handbook of Optics, 29.

α. The classical experiment is performed as follows. Set up in front of a lamp flame a black screen, having a narrow slit, behind which is fixed a piece of cobalt-blue glass. Red and violet rays come through the glass to the observing eye. Accommodate for a point behind the spot of light, and you see a red spot with violet halo; accommodate for a nearer point, and you see a violet spot with a red halo. Find the point at which the violet rays converge as far before the retina as the red converge behind it, *i.e.*, the point at which the spot appears uniform in colour. — Helmholtz, 158 f.

β. Paste a small square or circle of white paper upon a ground of dead black cardboard. Accommodate for a point behind the figure, and the margin is tinged with reddish-yellow; accommodate for a point before it, and the margin is tinged with blue. Accommodate for the margin, and there is no coloured fringe.

With the eye thus accommodated, bring a card close before it, to cover about half the pupil. If the card comes from the black side of the margin, there is a yellow fringe; if it comes from the white side, a blue fringe. — Helmholtz, 159 f.

γ. The phenomena (mixed with those of irregular astigmatism) are shown with astonishing vividness by the accompanying diagrams. Walk backwards from the larger figure, so that it comes to lie farther and farther beyond the range of exact accommodation. Bring the smaller figure in towards the eye, until it is too close for exact accommodation. Give a precise description of the colours seen. — The figures were devised by W. von Bezold, professor of physics and meteorology at Berlin (b. 1837). See Arch. f. Ophthalmologie, 1868; Helmholtz, 162; *cf.* Sanford, exp. 109.

(*c*) *Spherical Aberration: Monochromatic Aberration, Astigmatism.* — α. In good optical instruments, the lenses are 'aplanatic,' *i.e.*, have been corrected for spherical aberration. This is not the case in the eye. Spherical aberration could, however, play but little part in vision: partly, because the iris cuts off peripheral rays; partly, because the lens is not a homogeneous refracting medium, but is composed of layers increasing in density, and therefore in refracting index from outer surface to centre, — a complexity of structure which cannot, of course, in itself be regarded otherwise than as a defect of the optical system of the eye. — Suter, 28, 64.

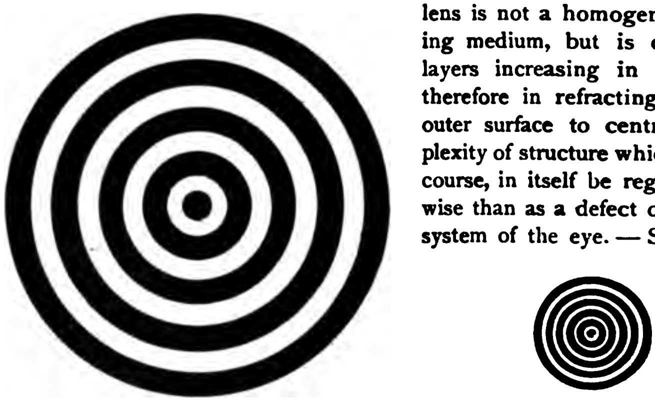


FIG. 50. — Helmholtz, 162.

β. If, however, there is no appreciable spherical aberration in the eye, there is a marked degree of deviation asymmetrical to the optic axis (irregular astigmatism), which would not be found in a well-made optical instrument. Prick a fine pin-hole in a black cardboard screen, and look through it at a bright white surface. Set the pin-hole, first, beyond the point of distinct accommodation (using a convex spectacle-lens if necessary), and, secondly, at a point nearer than that for which the eye is accommodated (using a weak concave lens if necessary). Draw and colour the dispersion images obtained in the two eyes. — Helmholtz, 170.

γ. There is, further, a very grave defect which is found in some measure in almost every eye, — the aberration known as regular astigmatism. It may be

occasioned either by difference of the curvature of the refracting surfaces in different directions, or by faulty centering of spherical surfaces. We know that the eye is badly centred ; and it has been found, also, that sections of the refracting media through the vertical and horizontal meridians show different radii of curvature. The cornea, especially, is apt to present a toric, not a spherical surface.

The commoner case is that the curvature of the vertical meridian of the cornea is greater than that of the horizontal. In this event, accommodation for a horizontal line means under-accommodation for a vertical, and *vice versa.* Either line can be seen distinctly by itself, but the two cannot be seen distinctly if they are together in the same plane. Test by fixating the centre of the diagram; and find by Scheiner's experiment the near point of accommodation for a vertical and horizontal pin. —Helmholtz, 175; Fick, 108.

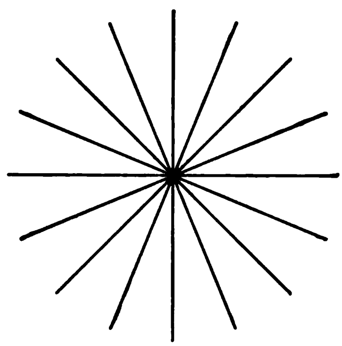


FIG. 51.—Helmholtz, 175.

δ. It may be added that the substance of cornea and lens is not quite transparent. If a bright light be seen against a dark background, it is surrounded with a whitish nimbus, which is brightest in the immediate neighbourhood of the flame. Cover the light by a screen brought before the eye, and the nimbus disappears. — Helmholtz, 178.

(*d*) *Imperfections of Accommodation, Errors of Refraction.*—Here belong the optical defects known as myopia (short sight) and hypermetropia (long sight). The myopic eye is too long for the lens, or the lens too thick for the length of the eye. A dispersive (biconcave) lens is needed to remedy the fault. The hypermetropic eye is too short for the lens, the lens not thick enough for the focussing of near objects on the retina: the remedy is a collecting (biconvex) lens.—Suter, 96 f.

(*e*) *Imperfections of the Refracting Media, Entoptic Phenomena.*—We have seen that the lack of transparency of cornea and lens leads to a form of monochromatic aberration. There are various other disturbances and imperfections of the optical system, temporary or permanent, which show themselves in the field of vision. These appearances are grouped together under the general title of 'entoptic' phenomena. The chief of them are as follows.

α. Drops of moisture running down over the corneal surface, and specks of dust or mucus caught in the corneal film, appear as bright cloudy streaks and bright-centred circles respectively. They disappear with winking.

β. Crinkling of the corneal surface by 'rubbing the eyes' gives rise to systems of wavy or criss-cross lines, or speckled patches.

γ. Impurities in the lens or its capsule are seen as pearly drops, dark specks, and bright, irregularly stellate forms. Sometimes a dark radiate figure comes up, corresponding to the structure of the lens.

δ. Impurities in the vitreous humour produce the well-known *muscae volitantes*. They appear as large, separate circles; strings of pearls; clusters of bright and dark circles, of various sizes; bright ribands with dark borders, etc.

ε. Retinal circulation. Look steadily (with the naked eye or, preferably, through a blue glass) at a bright cloud, or a misty summer sky. You see numbers of bright yellowish points moving quickly across the field, apparently at random. Longer observation shows that the tracks are permanent. The phenomenon is due to the temporary clogging of the retinal capillaries by large blood corpuscles.

For these and kindred phenomena, and the shadow-method of observation, see Helmholtz, 184 ff.; Sanford, exps. 110–112; Foster, 1189 ff.

(4) *Eye Movements.* — The best account of eye movements, from the psychological side, is that given by Wundt, Phys. Psych., 4th ed., ii., 109–124, 164–173. It owes much to Hering's Die Lehre vom binocularen Sehen (1868). This book, the same author's later exposition in Hermann's Handbuch der Physiologie, iii., 1, 1879, 437 ff., and Helmholtz' section (Physiol. Optik, 613–669), should be read by all advanced students: they are too elaborate and technical to be given as general class references. See also (especially for diagrams) Foster, 1277–1291; Waller, 429–434.

QUESTIONS. — (12), (13) See references above. A clear notion of false torsion may be obtained from Waller, 430 f. The 'orientation' of the eye is its determination to the points of the compass; more specifically, its position with regard to objects in the field of vision.

(14) The tendency (Sanford, 120) is that "to move in such a way as to bring any bright image lying on a peripheral part of the retina . . . into the area of clearest vision." See Wundt, ii., 171, who brings the reflex tendency into relation with the laws of innervation of the double eye; *cf.* Hering, Bin. Sehen, 23 ff.

(15) See Wundt, 166; Hering, Bin. Sehen, 3; Hermann's Hdbch., 520.

(16) The laws of F. C. Donders (a Dutch oculist, 1818–1889)

and of Listing (p. 232 above) are differently estimated and differently placed in the different systematic discussions of eye movement. Wundt recognises, besides the psychophysical law of the correspondence of apperception and fixation (p. 234 above), three physiological laws of movement: the law of preference of the primary position, the law of simplest innervation, and the law of constant orientation. The last-named is the law of Donders. All three may be subsumed under the general principle of 'movement with the least expenditure of muscular effort'; and Listing's law thus becomes merely a mechanical consequence of the law of simplest innervation. — Phys. Psych., ii., 116, 120 f.; Helmholtz, 669.

Helmholtz and Hering (Beitr. z. Physiol., iv., 2, 260 f.) incline to admit the factual validity of Wundt's general principle, though they do not consider it fundamental. Donders' law is, for Helmholtz, the principle of easiest orientation for resting positions of the eye; and Listing's law is the first logical step beyond, the solution of the first specific problem falling under, this principle. Hering names Donders' law 'the law of same retinal position with same position of regard,' and brings Listing's law into immediate connection with it under the title 'law of orientation with parallel lines of vision.' — Phys. Optik, 619 ff.; Bin. Sehen, 56, 63.

It is well that the student should understand the two laws in relation, and we shall therefore follow Helmholtz and Hering in our order of treatment.

Both laws are laws of eye movement; both presuppose a parallel position of the lines of regard; and both are formulated in terms of torsion. The difficulty of stating them — as, indeed, the wider difficulty of their systematic treatment — lies very largely in the fact that neither was fully worked out by its author (Hering, in Hermann's Hdbch., 474; Helmholtz, 669). We may phrase (1) Donders' law as follows. 'Given the position of the line of regard in relation to the head, and you have given with it a definite and invariable torsion value' (Helmholtz); or 'The orientation of the eyes is an univocal function of the position of regard' (Hering); or 'The orientation of the eye for any position of

the line of vision is constant, no matter by what path the line of vision may have been brought to this position' (Wundt). That is to say: if you set out with the lines of regard parallel, you may move from any position you like to any other position that you like in the field of regard, and the orientation of the eye in this second position will always be the same, whether you travel to it by a straight road or by a road the most devious and complicated possible. You might conceivably have any number of changes in orientation, corresponding to different degrees of torsion (Wundt, 120); so that your orientation might never be precisely the same from one movement to another. Or you might, of course, have determinate torsions correlated with certain general directions of the lines traced by the point of regard (cf. Helmholtz, 637). As a matter of fact, orientation is constant: as you were when you got to the new fixation-point the first time, so will you be when you get to it the hundredth time, and you may choose your own road.

(2) Listing's law implies not only a parallel position of the lines of regard, but also the primary position of the eyes. It may be phrased as follows. 'If the line of regard travel from the primary to any other position, the torsion of the eyeball in this second position is the same as if the eye had turned about a fixed axis at right angles to the first and second directions of the line of regard' (Helmholtz); or 'In movement from the primary position, the line of vision can describe a plane path, or the regard travel along a straight line, in any direction whatsoever, without there being any torsion of the eye at all about the line of vision: in other words, the eye can be turned in all directions about a fixed axis, at right angles to the line of vision' (Hering); or 'All movements from the primary position take place about fixed axes, each of which cuts the plane, described by the line of vision in turning, at right angles in the point of rotation, and all of which lie in a single plane, cutting the primary position of the line of vision at right angles in the point of rotation' (Wundt). That is to say: if you set out with the lines of regard parallel in the primary position, you may move to any point of

the field of regard that you like, in the vertical, horizontal or oblique direction, and your eye will undergo no torsion at all. Not only is the orientation constant: not only, *i.e.*, may you presuppose a definite and invariable torsion value: the torsion value is zero.

We now proceed to put these laws to the test of experiment.

MATERIALS. — Head-rest, with mouth board and sighting mark. [The latter is figured by Helmholtz, 657. The board *A* is 43 by 4 cm. The semicircular cut is smeared with hot sealing wax; *O* bites into this, before it is cold, the impression of his teeth, enabling him always to take up precisely the same position in the head-rest. *B* is a wooden upright, and *C* a strip of card or stiff paper, stuck to *B* by wax. The length of *C* must be made equal to the interocular distance (the distance between the points of rotation of the eyes). This distance is determined as follows. Fixate a point upon the remote horizon. The paper strip appears in double images. The strip must be so adjusted, and its ends so clipped, that the inner edges of the two images just touch in the same straight line. The length of the strip then corresponds to the interocular distance.]

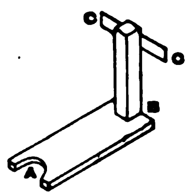


FIG. 52. — Helmholtz, 657.

Large grey or white screen, ruled in black at equal distances with vertical and horizontal lines. Two strips of red cardboard, with pins for fastening. [As to the dimensions of the screen, one can only say — since parallel lines of regard are required — 'the larger the better.' The end-wall of a large lecture room may be utilised, by hanging strips of black tape, weighted at their lower ends, from the picture-moulding, and tacking other strips horizontally and diagonally, as the experiment calls for them. To set a lower limit: a screen of 2 m. square can probably be accommodated in the smallest lecture room, and with an ordinary head-rest will answer the purpose of the experiment fairly well. It should consist of white cotton cloth, stretched over a light wooden frame; the lines are made as required by

pinning-on lengths of black tape. The coloured strips may then be cut 15 by 1.5 cm.]

Sanford recommends (Lab. Course, 122) the use of a quarter-screen (left-hand upper quarter), containing a vertical, a horizontal, and a prolonged oblique radius. The author has had this screen made of white holland, and mounted on a 10 cm. spring roller, like a heavy window shade. The screen is 3.5 by 2.6 m.; the vertical, horizontal and oblique lines, and their fixation points, are painted on the cloth (vertical and horizontal radii, 2.15 m., oblique line, about 3 m.). A light green disc of 30 cm. diameter, carrying a cross of deep red lines, 2 cm. wide, can be turned in a brass eye placed at the point of intersection of the three lines. The observer is stationed at a distance of some 8 m. This apparatus works satisfactorily, is very compact, and can be installed for $15.00. The classical form of the experiment, with the complete (not quarter) screen, is, however, preferable.

EXPERIMENT (1). — Donders' Law. *O* stands as far as possible away from the ruled screen, and secures his head in the head-rest by biting into the mouth board (the wooden upright and paper strip are removed). *E* places the red cardboard strips at right angles to each other over the point of intersection of a vertical and horizontal line upon the screen. *O* fixates the centre of the red cross, steadily, until a clear after-image is set up. He then projects the after-image to some point upon the screen, and carefully observes its appearance and position with regard to the nearest lines. If necessary, *E* marks the outline of the after-image upon the screen, *O* indicating its position to him by finger movements agreed upon beforehand.

This done, *O* returns to the red cross, and again waits for the formation of a clear after-image. Having obtained it, he moves his eyes at random, over side-walls, ceiling, etc., and only after such excursion brings the after-image to rest at the place upon the screen chosen in the first part of the experiment. The new after-image may 'hesitate' for a couple of seconds, but almost immediately takes up the precise position occupied by its predecessor (Helmholtz, 623 f.; Hering, Bin. Sehen, 98).

Hering's experiment with the rotated stereoscopic half-picture may well be performed here: Bin. Sehen, 57.

(2) Determination of the Primary Position. This position (as stated by Listing and confirmed by Helmholtz) varies but little for the emmetropic eye from that in which the head is held upright in its normal attitude, and the eyes are fixed upon a remote point at their own level above the horizon; so that the lines of regard are parallel and directed straight forwards in the horizontal plane. It differs somewhat, however, for different observers, and even for the same observer at different times. And, as a rule, the plane of regard is slightly depressed below the horizontal plane. — Aubert, 608, 654 f.; Hering, Bin. Sehen, 44, 64; Hermann's Hdbch., 471; Helmholtz, 626; Wundt, 114.

O sits at the far end of the room, his eyes directly opposite the point of intersection of the vertical and horizontal lines at the centre of the screen. The position of his head is fixed by the head-rest and mouth board, which latter now carries the sighting mark. *E* places a red strip horizontally at the centre of the screen. *O* closes one eye, and looks with the other, past the corresponding end of the sighting mark, towards the red strip. When an after-image has developed, he moves it straight up and down, and straight in and out, and observes whether it coincides with the horizontal lines of the screen. If it does, the primary position is found. If it does not, the position of the sighting mark must be corrected. Suppose that, as *O* looks up, the left end of the after-image is the higher; and, as he looks down, the left end is the lower: then the paper strip *C* must be pushed to the left. If the image has the opposite inclinations, the strip must go to the right. Suppose that, as he looks to the left, the left end is the lower; and as he looks to the right, the right end: the strip must be pushed up. In the opposite case, it must go down.

Repeat the experiment with the other eye, and then with both eyes open. — Helmholtz, 658; Aubert, 654; Hering, Bin. Sehen, 74 ff.; Hermann's Hdbch., 471 ff. Notice Hering's caution that "the more carelessly one works, the prettier is the [apparent] agreement with the law."

(3) Listing's Law. *O* puts his eyes in the primary position. *E* fixes the two red strips upon the vertical and horizontal lines

intersecting at the centre of the screen. *O* obtains an after-image of the rectangular cross, and projects it straight up and down and straight in and out, from the primary position. The vertical limb of the image coincides with the central vertical line of the screen; its horizontal limb coincides with the central horizontal line. There is, therefore, no torsion.

The horizontal and vertical tapes of the screen are now unpinned and pulled aside, and two diagonals stretched. [If the small screen is used, it is turned up on end, through 45°.] The red cross is placed upon the diagonals, so that its limbs lie obliquely. *O* gets an after-image, and projects it along the diagonals. The oblique lines of the image coincide with the black lines of the screen. Again, then, in these oblique movements from the primary position, there is no torsion.

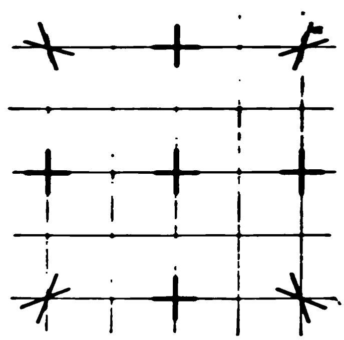


FIG. 53. — Hering, Bin. Sehen, 67.

The screen is restored to its previous form, with the cross placed rectangularly. *O* gets an after-image, and projects it obliquely, say, to the upper right-hand point of intersection of a vertical and a horizontal line upon the screen. The after-image does not appear as a rectangular cross, but assumes the appearance and position shown at *a* in the diagram. The change cannot be due to torsion; oblique movements from the primary position have just been shown not to produce torsion. Moreover, no amount of torsion could produce this particular change; for the vertical limb is turned *out*, while the horizontal is turned *in;* and torsion would have turned both in the same sense. To what, then, is it due?

Is it due to what is called a false torsion, or torsion of projection. If the line of sight is directed to the point *a* of the figure, the vertical and horizontal lines that intersect at this point are projected on the retina in the directions of the lines *hh* and *vv* in Fig. 54 *A*, while the after-image remains rectangular, as the

cross in the same Fig. shows. But we still see the lines of the screen as rectangular, *i.e.*, we see all the angles *vah* as right angles, because we refer their distorted retinal image not to a field of vision that cuts the direction of regard at right angles, but to a field that lies parallel to the frontal plane. It follows, then, that we must see the really rectangular after-image of the cross in the position indicated in Fig. 54 *B*. The apparent distortion of the after-image, its 'false torsion,' is due to the fact that we persist in seeing the vertical and horizontal screen-lines as vertical and horizontal, despite their oblique projection on the retina. — Hering, Bin. Sehen, 68; Hermann's Hdbch., 486; Wundt, 118; Aubert, 656; Helmholtz, 622, 658.

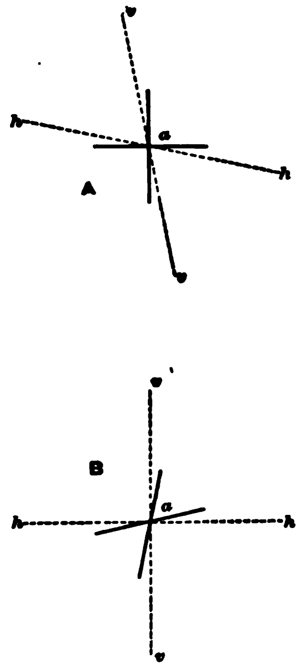


FIG. 54.—Hering, Bin. Sehen, 68.

This whole experiment must be performed with each eye singly. For there is, plainly, a possibility that compensatory torsions of the two eyes occur, in which case ordinary observation would take the after-image to be coincident with the vertical and horizontal screen-lines, and only a very keen and accurate discrimination would discover the double images and their intersection at a small acute angle (Hering, in Hermann's Hdbch., 474). It may then be performed with both eyes open.

It is an evident corollary to the law and its experimental verification as given above, that if the line of vision is to describe a plane path, not from the primary but from some secondary position, the eye cannot turn about a fixed axis at right angles to the line of vision, *i.e.*, cannot turn without torsion, save in the single case that the plane in which the line of vision moves is a plane passing through the primary position. Suppose, *e.g.*, that the head is inclined straight downwards from the primary position, and the after-image of the horizontal

strip is obtained with the eyes raised. Projection of the image on the horizontal meridian of the screen, to right or left, shows an increasing torsion as the line of regard travels from the original fixation-point. Suppose, now, that the after-image of the vertical strip is got with the same position of eyes and head. This time, the after-image, as the eyes carry it up or down, remains in the vertical screen-line: the line of regard is moving in a plane which contains the primary position (plane of the primary vertical meridian: Hering. Bin. Sehen, 66). If, on the other hand, the image is carried along the horizontal meridian, it does not remain perpendicular to it, but shows traces of torsion. — Repeat these observations, with the head turned to right or left, and down upon the right or left shoulder.

We need not, however, move the head, in order to obtain a secondary position: it is sufficient to move the eyes. Think of our screen as divided into six vertical columns of equal width by seven vertical tapes. Let a red strip be fixed vertically at the centre of the middle line; and let the eyes be set over against the centre of the strip in the primary position. If we move the after-image straight up or down, and straight in and out, there is no torsion. But if we move it a step out, and then up; two steps out, and then down; and so on, then we get a torsion. By moving always only in the horizontal and vertical directions we avoid the false torsion of projection, and get a true picture from the after-image of the direction of torsion (Hering. Bin. Sehen, 70).

There are two important geometrical corollaries to the law, which may be worked out by students if time permit: see Question (24) below. They are as follows. (1) All the axes about which the eye can begin to turn from the primary position lie in a single plane. The same thing holds of movement from any secondary position, only that in such cases *the axial plane has a different position to the line of vision*. (2) The eye can turn about a fixed axis from any given secondary position to any other secondary position. If the fixed axis is one of the primary axes, so that the line of vision must pass through the primary position, the path described is a plane path. If the positions passed through are *all secondary positions, the line of vision describes a curved path*, in general, a path upon the surface of a cone.

For deviations from the laws of Donders and Listing, with convergent lines of regard, see Hering, Bin. Sehen, 58, 92 ff.; Hermann's Hdbch., 496 ff.; Helmholtz, 625 ff., 659, 664; Aubert, 658 ff.

Sanford has constructed a model of the hemispherical field of regard, projection within which avoids the false torsion of the plane field. Since, however, the hemisphere is so small that the vertical meridians are not parallel for vision, but converge sensibly above and below, a new (though smaller) false torsion is introduced (Lab. Course, 423). When the hemispherical is converted into a plane field, by gnomonic projection upon a plane tangential to it at the middle point of the central cross, this new false torsion is, of course, superadded to the false torsion of plane projection which we have been discuss-

ing (*ibid.*, 425 f.). Notice that Sanford uses the phrase 'rotation of the eye about the line of regard' for what is here termed 'torsion,' and the word 'torsion' in the sense of *Helmholtz'sche Raddrehung* (Aubert, 657) for what we have called 'false torsion.'

For methods and apparatus more accurate than those given in the text, see Helmholtz, 659; Hering, Bin. Sehen, 78. For the Hering method of substitution, see Bin. Sehen, 83 ff.: Hermann's Hdbch., 480 ff.; Helmholtz, 662 f.; Aubert, 647, 649.

What, now, is the good of these laws? What is their optical significance? The question will, as we said at the outset, be differently answered, according to the stress laid upon the one or the other law in an author's systematic treatment of binocular eye movement. Helmholtz looks upon Donders' law as a guarantee that resting objects in the field of vision are recognised as such, *i.e.*, are seen to be resting objects, when the eye itself has been moved (638). "The observance of this law must contribute essentially to the ease and certainty with which we solve the problem of recognising unmoved objects on the retina as unmoved, despite the movements of the eyes and despite the displacements of the retinal images" (637; *cf.* 638, footnote). Listing's law is then shown to be the most advantageous law of eye movement, so far as orientation is concerned, first of all for monocular vision and a circular field of regard, and then for the binocular field with parallel lines of vision (642 f.). Hering estimates Listing's law as follows. "It brings the space perception [localisation] of the moved eye into the greatest possible unison with the perceptions [localisations] of the resting eye," so that "the displacements of the retinal images harmonise with the intended movements of regard" (principle of avoidance of torsion, or of 'apparent movement' of objects in the visual field); while it also assures in far vision "the most perfect possible correspondence of the retinal images of the double eye" (principle of the greatest horopter): Bin. Sehen, 106 f.; Hermann's Hdbch., 539, *cf.* 503. He brings Donders' law under his 'principle of simplest innervation' in eye movements: Bin. Sehen, 56, *cf.* 32 ff.; and see esp. Beiträge z. Physiol., iv., 2, 1864, 248 ff. The reconciliation of these three principles in practice is discussed in the Beitr., *loc. cit.*, 269 ff. Wundt, as

we saw above (p. 243), subsumes all the laws of eye movement to the principle of least expenditure of muscular effort; A. Fick, in articles published in 1854 and 1858, did the same thing. Le Conte (Sight, 1881, 164 ff.) accepts Listing's law as the law of parallel movements, but declares that in convergence "the law of Listing is wholly abrogated, or else overcome, and another law [that of outward rotation] reigns in its place." *Cf.* Aubert, 669 f.

(5) *Corresponding Points and Double Images.* — See Helmholtz, 841 ff.; Wundt, ii., 173 ff.; Hering, in Hermann's Hdbch., iii., 1, 343 ff.; Beitr. z. Physiol., esp. i., 1861, 22 ff.; iii., 1863, 184 ff.; Foster, 1275-1277, 1291 f.

QUESTIONS.—(17) The terminology of the 'points' is from Wundt, q. v. It is not necessary that the student, at this stage, should know more of the horopter than is given in Waller, 426 f., or Foster, 1291 f.

(18) There are several methods for this determination. The most satisfactory is, perhaps, that finally recommended by Helmholtz, which may be given in his own words. It presupposes practice in 'parallel vision.'

"A sheet of black paper is stretched upon a vertical wooden board. Upon the paper are fastened, side by side, (*a*) a strip of red paper, 3 mm. wide and bounded by two straight parallel edges, and (*b*) a blue thread. Strip and thread are set almost vertically, diverging a little from below upwards. The distance between them, at the height of the eyes of the observer, is equal to the distance between his two eyes. The strip is fastened at both ends, the thread only at the upper end; it is kept taut by a small weight attached to its lower end. The observer pushes the lower end of the thread sidewards with a pin, as far as is necessary, and finally sticks the pin in the board when the thread has come to the right position. He looks at strip and thread with parallel lines of regard, so that the blue thread appears in the middle of the red strip, and moves the thread until it seems to lie along its whole length precisely in the middle of the strip. Then he sticks the pin in the board. By measuring the distance of the thread from the strip at its upper and lower ends, and the vertical distance between these two points, he can easily determine the angle required." — Phys. Optik, 851; *cf.* 687 f.

See Hering, Beiträge, iii., 1863, 175 ff.; Hermann's Hdbch., iii., 2, 355 ff., 368 f.; Wundt, Phys. Psych., ii., 140 ff.; Aubert, Phys. Optik, 608 f. Sanford gives a simple and pretty experiment (Lab. Course, 268 f.) by a method which appears to have been suggested by Meissner, and was later modified by Volkmann, Helmholtz and Hering.

(19) The two fingers, held out on the same line of vision, or two pegs on a metre stick, answer very well. The diagrams for double images of this kind are given in Figs. 55, 56. In the former, the fixation-point is constant, and the object varies its position; in the latter, the object is constant and the fixation-

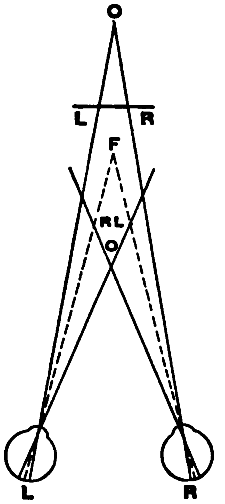


FIG. 55.

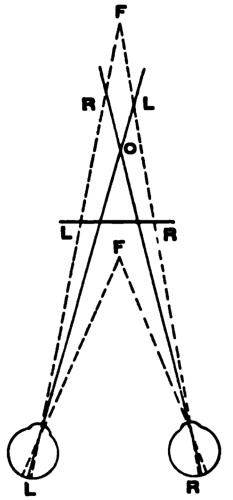


FIG. 56.

point varies. Students who do not carry these diagrams 'in their heads' by visual memory are apt to be confused by the occurrence in books now of the one and now of the other figure. They may be helped by the mnemotechnic lines:

Remote regard reverses;
Nearer notice, not.

Remember that other diagrams are to be drawn, in which the non-corresponding points fall, not upon the two nasal or two temporal retinas, but upon the nasal side of the one and the temporal side of the other.

It was a dogma of the older literature of physiological optics that double images are always seen in the plane of the fixation-point. Hering has insisted, and Helmholtz agrees, that double images are seen for the most part fairly accurately, *i.e.*, in a plane not far removed from that of the object which gives rise

to them. This plane is somewhat variable; it lies always between the planes of object and fixation-point. With continued steady fixation, and in the absence of all empirical criteria, it practically coincides with the latter.—Wundt, 178; Helmholtz, 868; Hering, Beitr. z. Physiol., v., 1864, 335; *cf.* ii., 1862, 142 ff.; Hermann's Hdbch., 427.

In Scheiner's experiment, we made two images of a single object fall on different parts of the same retina; here we make two images of a single object fall on non-corresponding parts of the two retinas. The principle of crossed and uncrossed images is the same in both cases.

(20) This (imaginary) statement is given as typical of the statements which the student is likely to meet in text-book or other brief expositions of the theory of binocular vision. It may be so interpreted as to be correct; as it stands, it is very misleading. Fifty years ago, facile reference to 'experience' and 'empirical motives' was permissible. The factors that enter into 'experience' had not been analysed out. Nowadays, we have to deal, not with 'experience' in the large, but with a number of special facts (facts of organism and facts of environment), which take on a different colouring and a different relative importance according to the general theory in which they find a setting. Hence the student, after doing what he can to estimate the statement quoted, should be referred to Wundt's elaborate argument on pp. 179–184, to Helmholtz' chain of deduction on pp. 948 f., or to Hering's masterly summary in Hermann's Hdbch., 424 ff. The lesson to be learned is, that if one talks of 'experience' one must have a very clear and very detailed notion of what 'experience' means. It is no more 'scientific' to 'explain' a given phenomenon by referring it to an indefinite experience, than it is to 'explain' it by reference to an unconditioned and indeterminable faculty of will.—Limits of space forbid the working-out of an illustration. Some one of the above instances, however, or a similar instance chosen by the Instructor, should by all means be worked out by the student.

ADDITIONAL QUESTIONS.—(21) "Not only the more general movements of the eye which obey Listing's law, but also those

which form an exception to it, appear to be carried out in the interests of binocular vision" (Foster). Work out the cases in which torsion occurs, and test this statement.

(22) Make three pin-holes .·. in a card, within a space smaller than the extent of the pupil. Bring the card close up to the pupil. Some 2 or 3 cm. before it hold another card, pierced with a single pin-hole. The triangle appears as ·.·.

Hold the second card some 3 or 4 cm. before the pupil. Bring up the head of a pin, close to the pupil. You see a large, shadowy inverted pin in the circle of light.

Explain these two results.

(23) Seat yourself at about 50 cm. distance from a window commanding a wide prospect. Secure the head in a head-rest. Close the right eye. Select with the left eye some prominent object in the field (a tree, *e.g.*), lying a little to your right. Make an ink-mark on the window pane, covering the centre of the tree as seen by the left eye. Now close the left and open the right eye. Notice what object in the field (a chimney, *e.g.*) is partially covered by the ink-mark. Finally, open both eyes, and fixate the ink-mark. Directly behind it, and partly covered by it, you see both tree and chimney; in other words, mark, tree and chimney lie in the same direction. — Explain this result. See Hering, Hermann's Hdbch., iii., 1, 386 ff.; Höfler, Psychologie, 291 ff.

(24) Define: circles of direction (right circles), occipital point, atropic line. — Sanford, 424; Helmholtz, 651, 678; Hering, Bin. Sehen, 73; Hermann's Hdbch., 490 ff.

We have now, at least in essentials, fulfilled Brewster's requirements of those who enter upon the study of stereoscopic vision. And the Instructor, so far from extending the exercises to greater length (p. 232), may very well object that work of this sort is physiological, or at best psychophysical; not psychological at all. Could we not get on, in psychology, without it? Do we ever really make use of this cumbrous terminology? Is it worth while to take the student back to the times when controversy raged about the horopter and the projection theory and the doctrine of identity?

The answer is the same, whether we appeal to authority or to experimental work. None of the men who have erected 'theories' of space perception have failed to go through the mill; all the expositions of such theories take the reader through it, before the psychology is reached. The same thing holds of experimentation; you cannot set a student to work upon the stereoscope, with any hope of intelligent results, unless the preliminary matters which we have been discussing are as familiar to him as the alphabet. The author wrote out Exp. XXVII. before he wrote the present Section, and jotted down as he went the points that called for a preceding explanation. No single topic has been introduced here that was not directly suggested by the course of the Experiment; no single test has been given merely 'for the sake of completeness.' As for the direct bearing of the preliminaries upon psychology proper, upon space theory, it will perhaps be enough if the student, at this stage, realises how great is the complexity of the problem, how immense the collection of observed facts, and how imperative the need of accuracy. He need not attempt to form an opinion of his own until he comes to systematic work, at the conclusion of this Course. It is very much better that he should acquire knowledge and suspend judgment.

Even for the mature psychologist, judgment is sufficiently difficult. We find Hering declaring that the eye-movement theory of the depth perception 'turns things upside down,' and Aubert and Donders saying that one of the essential propositions in Hering's own theory is 'only a periphrasis of the facts, but no explanation.' Wundt asserts that Hering gives 'forced explanations' and comes into 'conflict with observation'; Hering is no less emphatic on the point that Wundt has 'two views, which are without any question mutually exclusive, but which are none the less represented at one and the same time.' Wundt, again, finds that Helmholtz and Bain fail to overcome the difficulty inherent in all 'empiristic' theories, the difficulty that perception, the basis of experience, cannot itself rest upon experience. Hering, too, is very decidedly opposed to Helmholtz' doctrine of the empirical coördination of the two eyes for purposes of space perception. Helmholtz, on his side, appeals

confidently to experiments which show that Hering's hypothesis contradicts the facts, and regards Hering's 'Tiefengefühle,' in particular, as valueless.

All this means, simply, that (as Helmholtz says) "the questions here discussed are not yet fully ripe for discussion." It does not mean (and the Instructor should see that the student does not fall into any such mistake) that the psychology of visual space perception is nothing but controversy, "gossip and wrangle about opinions." The facts are given: the difficulty lies in coördinating and unifying the facts. Every 'theory' surmounts this difficulty for a certain proportion of the given material, or perhaps for all the material as considered under certain aspects. On the objective side, therefore, a theory serves as the point of departure for new investigations; and in this way the clash of theories is of extreme importance for the progress of science. On the subjective side, the theory furnishes an aid to memory, acts as a net to hold the facts together, while it also provides a working hypothesis, a code of provisional beliefs. The author has known students to be discouraged and disheartened by the divergence of expert opinion. 'If Helmholtz and Hering and Wundt, who have done so much, cannot agree,' they say, 'how can we hope to do anything?' But every one of us has the right to theorise, when he knows the facts; every one is assisted to such knowledge, meantime, by the existing theories; and every one may hope that, as the opposing theories grate and grind in his thought, they will at least strike out a few illuminating sparks, if they are not worn and rounded to a valid compromise.

EXPERIMENT XXVII

§ 47. **Stereoscopy.** *Cautions not noted in the Text.* — The language of this Experiment has been made as simple and as little technical as possible, in order to test the student's assimilation of the terms and definitions of the preceding Section. If these have been thoroughly mastered, the Experiment will fall into its technical setting of itself; if they have not, there will be hitches and difficulties in the work, which can be overcome only by a retracing of the path too hurriedly traversed.

This is the student's best introduction to the synthetic experiment, the nature of which should be made clear to him. Roughly defined, a synthetic experiment is one in which the products of mental analysis are artificially brought together, and the result of this recombination observed, in order that the exhaustiveness of the analysis may thereby be proved or disproved. By analysis we have reduced a tangle of processes to *a*, *b*, *c*; we now put *a*, *b* and *c* together, in the laboratory, and see if the original tangle results. If it does, the analysis was good; if it does not, the analysis was defective. The most satisfactory syntheses are, evidently, those in which the terms *a*, *b*, *c* are sensations, well known and strictly defined in other contexts: I. M. Bentley's reconstruction of the perception of liquidity, from elements that are not 'wet,' is an instance in point (Amer. Journ. of Psychology, xi., 405 ff.). The synthesis that has been most discussed in the literature of experimental psychology is, perhaps, that of the action consciousness in the 'reaction' experiment. In the present case, of the synthesis of relief by the stereoscope, we are, unfortunately, not able to get back to ultimate components: our terms are the plane pictures, spatially ordered, on the one hand, and the two retinas, connected with all the motor apparatus of the eyeball, on the other. Neither term is elemental. The pictures are obviously themselves perceptions, mental formations; and though we may exclude eye movement (movements of the eyeball as a whole, and movements of accommodation within the eyeball) by illuminating the stereoscopic slide momentarily, with an electric spark, yet we cannot rule out the motor dispositions of the eye which (on the eye-movement theory) may take the place of movements actually performed. The synthetic principle is, however, clearly brought out.

EXPERIMENT (1).—The importance of practice in 'free' stereoscopy, such as this experiment demands, is unquestionable. The experimental psychologist should have his eyes so far under control that he can fixate steadily (not so easy a matter as it may seem to be at first thought!), hold the lines of vision parallel in the absence of a remote fixation-point and despite the attraction of near objects in the visual field, and keep the eyeballs in any required 'squinting' position. All three acquire-

ments demand time and patience. It is noteworthy that (as Hering says: Bin. Sehen, 27) the setting of the eyes for near and far fixation need not be motived by any spatial idea. When one wishes to squint, one need only call to mind the 'peculiar feeling' of the inward-turning eyeballs, and the squint is realised. When one wishes to fixate an infinitely distant point, one need only 'let the eyes go,' give up the effort after clear vision, and 'push apart' the crossed double images. There is no necessity to ideate a very near object in the first case, or a very remote object in the second, natural as such spatial reference may seem.

Glasses should be dispensed with, if possible. Sometimes — as in the not rare cases in which the one eye is distinctly myopic, while the other is emmetropic or slightly hypermetropic — they must be worn. *O* must then see to it that they are correctly adjusted, *i.e.*, properly centred and parallel with the frontal plane.

It is probably true, at least of the students that one finds in laboratories, that convergent squinting is easier than the parallel position of the lines of regard (Hering), though in a mixed company preferences will be found on both sides. If the figures of the truncated cone, drawn as directed in this experiment, are handed round the class room, the reports as to the relief or hollowness of the combined image will differ with different individuals: all have taken the easiest path to combination, but for some this has meant far and for others near fixation (Ruete). There can be no doubt that near fixation gives the better effect (Le Conte); accommodation tends to follow fixation, so that the outlines of the combined image with parallel lines of regard are blurred and indistinct.

In view of the importance of the experiment, and of the existence of these individual differences, it is well to have a number of methods available. Hence we may cite some of the suggestions made by other authors, which the Instructor will perhaps prefer to the arrangement recommended in the text. We are thus anticipating the answer to Question (4). — (1) A piece of card or stiff paper, cut to fit the profile, and extending out about 25 cm. from the face to meet the card, will cut off the lateral single images in far fixation, and by confining each eye

to its own field of regard will assist *O* to obtain and maintain the required position of the lines of regard. Notice that, if the lines of regard are set in the parallel position beforehand, and the stereograms then interposed between the eyes and the remote fixation-point (this is the method usually adopted by beginners), fusion is easier when the card is brought down from above than when it is brought up from below the eyes. In near fixation, two small side screens, held upon a cross wire on which the near fixation-mark is fastened, replace the profile paper. (2) For distant fixation, the figures may be drawn upon glass or (more easily) upon celluloid, instead of card. The observer is thus able actually to look through the figures at the remoter point. Martius-Matzdorff (Stereoskopie, 8) advises the beginner to paste the figures on a window pane, and look through them at some fixed object in the street. Sanford (Lab. Course, 277) recommends a glass slide, with gummed kindergarten rings and dots laid on. Care must of course be taken that the distance between the centres of the figures does not exceed the interocular distance (Wundt, Phys. Psych., ii., 206 f.). (3) Helmholtz suggests the use of blackened tubes, and of simple stereoscopic drawings, whose centres are separated by less than the interocular distance (Optik, 784). Take two tubes of black cardboard, 20 cm. in length and 3 cm. in diameter. Make two cardboard rings, 1 cm. in width, to slide snugly over these tubes. Draw the two figures of the slide on a reduced scale (outer circle 2 cm. in diameter) upon architects' paper, and paste them over the rings. Fit the rings, or caps as they now are, over the ends of the tubes, with the figures in their appropriate positions, and hold the open ends of the tubes to the eyes. Bring the figure-ends of the tubes together, till the drawings fuse. Evidently, if a number of rings and figures be prepared, these and the tubes constitute a form of stereoscope. Its chief limitation is the small size of the pictures that can be combined. Münsterberg's Pseudoptics (p. xxxiii. above) contains two tubes and a set of caps.

The Figure shows two early devices of Wheatstone's (Phil. Trans. Royal Soc. London, 1838, 373), for far and near fixation respectively. The latter may be compared with Elliot's box stereoscope (described 1839: see Brew-

ster, Stereoscope); a needle is supposed to stand at the point of intersection of the lines R, L.

Le Conte (Sight, 129) recommends a skeleton truncated cone of wire, in place of the cardboard cone prescribed in the text. This is so far good that the lines to be drawn are all made directly visible to the student; but it is a question whether the gain in simplification is not more than balanced by the resulting mechanical nature of the drawing. The student is not called upon to see critically.

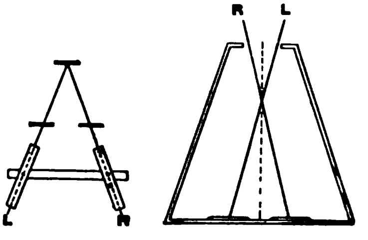


FIG. 57.

QUESTIONS.—(1) Each of the two figures is seen by both eyes, so that there are four single images before combination, and two single images and a total (combined) image after combination. Let us number the single images 1, 2, 3, 4 from left to right. Then, in far fixation, 1 and 3 belong to the right, 2 and 4 to the left eye; in near fixation, 1 and 3 belong to the left, 2 and 4 to the right eye. Nos. 2 and 3 combine; nos. 1 and 4 are therefore left outstanding.

(2) The difference of size will, in all probability, be remarked by all students in the one case or the other, though not with equal ease in both cases, and perhaps not at all in one of them. Wherever it is remarked, the combined image and the lateral single images do not lie in the same plane. In far fixation, the combined image seems to be farther off than the other two. There is a temptation to say, at the moment of fusing, that the frustum of the cone has leapt towards you from the plane of the card. This is not accurate: for the base of the cone has also left the plane of the card, and the solid figure stretches away from you. In near fixation, the central image approaches the observer. There is a temptation to say that the base of the cone has shot back; in reality, the whole solid figure has come in towards you. In other words, the combined image has tended, in both cases, towards the point of fixation. This may be seen very prettily if the card be moved out, after the eyes have been permanently set for near fixation. The hollow cone lies suspended in mid-air between *O*'s eyes and hand.

Now, if an object occupies the same amount of space as another object, and yet is farther off than this other, it must be the larger of the two. This is our first case. If, on the other hand, an object occupies the same amount of space as another object, and yet is nearer to us than this other, it must be the smaller of the two. This is our second case. All three images subtend the same visual angle, in both instances; but they subtend this angle at different distances from the eye, and appear of correspondingly different sizes. — G. T. Ruete, Das Stereoskop, 2te Aufl., 67 f. *Cf.* Wundt, ii., 201; Aubert, 628.

On the localisation of the combined image at the distance of the apparent point of regard, see Sanford, Lab. Course, exps. 211 *a*, 212 *b*. A striking experiment (Hermann Meyer) may be performed with an ordinary open-work cane-bottomed chair. Hold up the chair, the cane bottom parallel with the frontal plane, at about the distance of the near point of accommodation. Combine the octagons of the mesh-work by fixating, first, a remote object, and then an imaginary nearer object. In the former case, the somewhat indistinctly seen screen lies beyond the chair, — not, certainly, so far away as the distant tree or whatever it may be that forms the object of fixation, but still a great deal farther off than 12 cm.; in the second case, the screen comes out from the circular wooden rim, so that the chair bottom appears to be highly convex, its convexity towards the eyes. The comparative nearness of the screen with remote fixation is due to the counter action of secondary criteria. See Brewster, Stereoscope, 90 ff.; Aubert, 614; Helmholtz, 798 f.

(3) So far as the relation of the double images to their respective eyes is concerned, the drawings should present no difficulty. — In the three figures here given the combined image is placed always at the fixation-point. Fig. 58 shows that, if the point of fixation lie behind the two stereoscopic drawings, the corresponding points give a more remote combined image, the farther apart they are in the plane of the card: *i.e.*, what is drawn raised is seen raised, and what is drawn depressed is seen depressed. Fig. 59 shows, conversely, that if the point of fixation lie before the slide, the corresponding points give a more remote combined image, the nearer together they are in the plane of the card: *i.e.*, what is drawn raised is seen depressed, and what is drawn depressed is seen raised. The same result could, evidently, have been obtained if we had cut the slide of Fig. 58 in halves, interchanged the halves, and fixated a remote

point. Or, we may bring the slide of Fig. 59 back again to normal depth values by cutting it in halves, interchanging the halves, and maintaining the near fixation-point. We then have Fig. 60. See Ruete, Das Stereoskop, 65 ff.

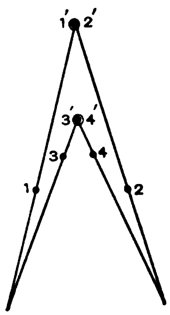


FIG. 58.

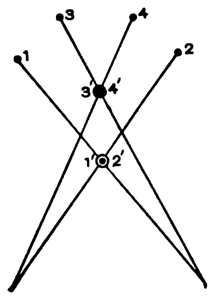


FIG. 59.

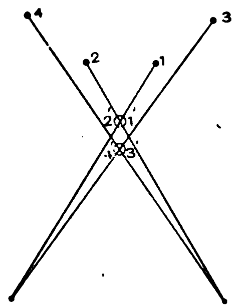


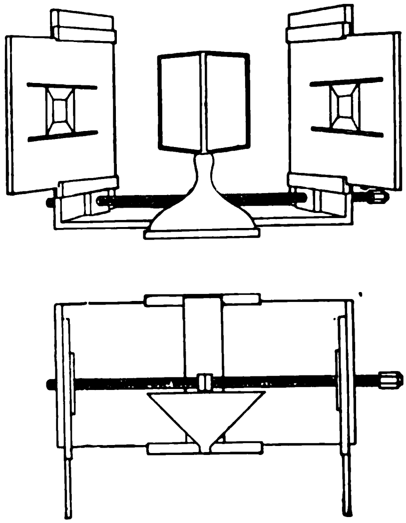
FIG. 60.

(4) The answer to this question has been given above, pp. 259 f. The student may very well think of (1) screens and artificial fixation-points; (2) transparent slides; (3) tubes. Another, much less obvious device, is Münsterberg's stroboscopic stereoscope, figured and described in the Psychol. Review, i., 1894, 56 ff.

The Stereoscope. — The student must be familiar with Wheatstone's reflecting stereoscope, as well as with Brewster's refracting stereoscope. We will begin with the former, which also has priority in date of invention.

The first mention of Sir Charles Wheatstone's (1802–1875) discovery occurs in 1833, in the third edition (p. 288) of the Outlines of Human Physiology by H. Mayo, a colleague of Wheatstone's at King's College, London. Wheatstone's own description of the reflecting stereoscope is to be found in the Phil. Trans. for 1838, p. 375; Figs. 61 and 62 represent a front view and a plan of the instrument. The account runs in substance as follows. 'Two plane mirrors, about 10 cm. square, are framed, and so adjusted that their backs form an angle of 90°. They are fixed by their common edge to a vertical board, cut away to allow the placing of the eyes before the two mirrors. The base consists of two sliding boards, each of which carries an upright

side-piece; these side-pieces may thus be removed to different distances from the mirrors. To secure equal amounts of movement, a right and left handed wooden screw is passed through the lower parts of the side-pieces. The side-pieces further carry panels, to which the diagrams may be affixed in such a way that their corresponding horizontal lines are on the same level; the panels slide backwards and forwards in grooves on the side-pieces.' The optical principle of the instrument is clear from Fig. 63. *AA* are the two mirrors; *BB* the two panels; ccc and $c'c'c'$ the two diagrams. The rays fall upon the eyes *DD'* as if they came from *EE'*. In other words, we see at *EE'* the combined (virtual) image of the two figures. Ruete, Das Stereoskop, 71 f.



FIGS. 61, 62.

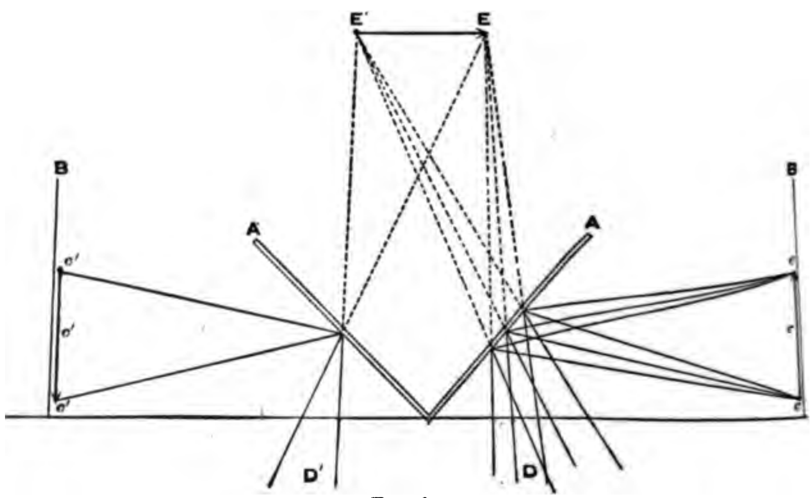


FIG. 63.

Figure 64 shows the later form of Wheatstone's reflecting stereoscope (Phil. Trans., 1852, 3). The sliding base-boards are here replaced by wooden arms moving round a common centre below the line of junction of the mirrors. The panels are run in and out on slides; the side-pieces are thus done away with. The diagram cards slip back and forth in grooves in the panels. "By the arrangement described, the reflected pictures are always perpendicular to the optic axes, and the corresponding points of the pictures, when they are exactly similar, fall upon corresponding points of the retinæ. The instrument has an adjustment for

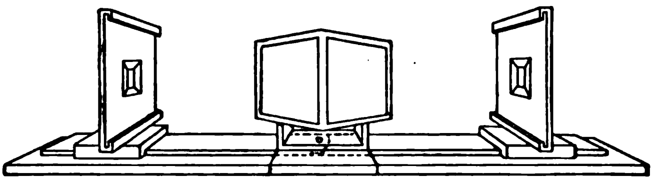


FIG. 64.

otherwise inclining them if it be required." Since the mirrors are fixed, this statement must mean that the panels can be rotated about vertical axes, as in modern forms of the instrument.

Sanford (Lab. Course, 408 ff.) gives specifications for the construction of a combined Wheatstone stereoscope and Helmholtz telestereoscope. The author has had this instrument built, and it works satisfactorily; though some of the moving parts might with advantage be made heavier. The cost is about $15.00. The design embodies Hering's improvement upon Wheatstone's model, that the lateral arms turn, not about a common centre, but about centres lying in the same vertical lines as the centres of rotation of the eyes (Hermann's Hdbch., 393).

An admirable instrument, of Hering's devising (built by R. Rothe), is figured and described by F. Hillebrand in the Zeits. f. Psychol., v., 1893, 38 and Plate i.

The Wheatstone stereoscope should always be so constructed that the panels are replaceable by stands to take wire-models instead of cardboard slides. The binocular image of two similar models will show relief, converted relief, or a plane picture, according to the position of the objects: Wheatstone, 1838, 378. For a device to throw the mirror images of a plane picture into relief, see Wheatstone, 1838, 378 and Fig. 21.

Let us see, now, how the apparatus works. The two diagrams to be combined are slid into the panels. The arms are set in

the same straight line, and the panels at an angle of 45° to the mirrors.

Push the slides well out upon the arms, at equal distances from the mirrors. Move the arms gradually outwards, away from you, till the images combine. The position recommended by Wheatstone as starting-point is that in which the binocular image appears of its natural size, *i.e.*, of the size of the diagrams. Any position will do, in which the eyes combine the diagrams easily and without strain.

Move the arms still farther out. The magnitude of the retinal images remains constant, but an increasing convergence is required if combination is to be maintained. Notice that the binocular image seems to grow smaller. Move the arms in again, towards you. Convergence is lessened, until (when the arms are in the same straight line) the lines of regard are parallel. There is still no change in the size of the retinal images. Notice, however, that the binocular image seems to grow larger.

Set the instrument again for easy combination. Move the slides steadily in towards the mirrors. Convergence remains constant; the size of the retinal images is increased. Notice that the binocular image seems to have come nearer. Move the slides outwards. Convergence is still constant; the size of the retinal images is decreased. Notice that the binocular image seems to have travelled farther away.

These two experiments are evidently of high theoretical importance. In the former, we have constancy of retinal image, constancy of accommodation, change of convergence. The apparent distance of the binocular image remains constant for ordinary observation, though careful examination may show that it has changed. At any rate, the principal and obvious result is that the binocular image *varies in magnitude*. In the latter experiment, we have constancy of convergence, change of retinal image, change of accommodation. The apparent size of the binocular image remains constant for ordinary observation, though it is not difficult to perceive the changes in size as such. The principal and obvious result, however, is that the binocular image *varies in distance*. What, then, are the criteria that make

against apparent change of distance in the first, and make for it in the second experiment? — Wheatstone, Phil. Trans., 1852, 3 f.; Helmholtz, 795 (a summary and not very exact account of the experiments); Sanford, Lab. Course, 285, exp. 217.

Combination with divergent lines of regard may be secured by bringing the arms in, very slowly and steadily, beyond the position of parallel vision. Helmholtz, using a series of similar figures the distance between whose centres was gradually increased, found himself able to combine divergently points 93 mm. apart, while his interocular distance was 68 mm. (800). *Cf.* Hering, Hermann's Hdbch., 507; Sanford, Lab. Course, 289, exp. 219*a*. We cannot here enter upon the special problem of divergent stereoscopic vision. It may be remarked, however, that the distance between conjugate points of the pictures in stereoscopic slides (Brewster's instrument) is often or even usually greater than the interocular distance. This fact has been interpreted to mean that the lines of regard must be strongly converged (crossed on the hither side of the stereograms) as we look into the instrument: *cf.* Ruete, Das Stereoskop, 66, and the diagram on p. 69. The interpretation is, doubtless, correct as regards the majority of instruments and slides used in laboratories for scientific purposes. W. LeC. Stevens has, however, recently shown, by measurements of the foreground intervals in commercial slides and the deviating power of the lenticular prisms in commercial instruments, that cases are not uncommon in which the rays from conjugate stereogram points are not quite parallel after emergence from the prisms, so that the eyes must diverge somewhat to receive them. Positions of divergence are especially to be looked for "among young persons whose interocular distance is small, whose eyes are normal, and whose power of accommodation, both focal and axial, is thus large" (Amer. Journ. of Science, 3 Series, xxii., 1881, 360, 444).

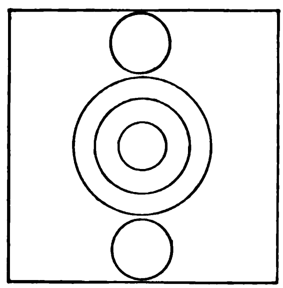


Fig. 65.

We have not yet availed ourselves of the rotation of the panels about vertical axes. The following experiment (LeC. Stevens) is instructive. Set the stereoscope for easy combination. Place in the panels two exactly similar slides, drawn as in Fig. 65. Turn the panels in such a way that they form a dihedral angle, opening towards the observer. The binocular image of the central circles is that of a convex surface; that of the two upper circles, an ellipse whose upper vertex is farther from and its lower vertex nearer to the eyes; and that of the lower two circles, an ellipse of reversed obliquity. Turn the panels, now, in such a way that their angle narrows towards the observer. The convex becomes a concave surface, and the inclination of the ellipses is the opposite of that in the former experiment. For most observers, the concavity is more marked and more readily perceived than the

convexity. Let the student work out an explanation of the phenomena.—Amer. Journ. of Science, 3 Series, xxiii., 1882, 298 ff., 359; xxiv., 1882, 243; J. Le Conte, *ibid.*, xxxiv., 1887, 103.

We have now to consider the refracting stereoscope of Brewster (1781–1868), which has driven the reflecting stereoscope out of general use. There seems to be no doubt that Wheatstone invented a prism stereoscope before Brewster thought of it (see The Edinburgh Review, Oct., 1858, cviii., 455; Encycl. Britannica, ninth edn., art. Stereoscope): Brewster's substitution of two semi-double-convex lenses for the two prisms, and similar improvements, date theoretically from 1843–4 (papers read to the Royal Soc. of Edinburgh, Jan., 1843, April, 1844), practically from 1849–50 (Encycl. Brit., art. Brewster).

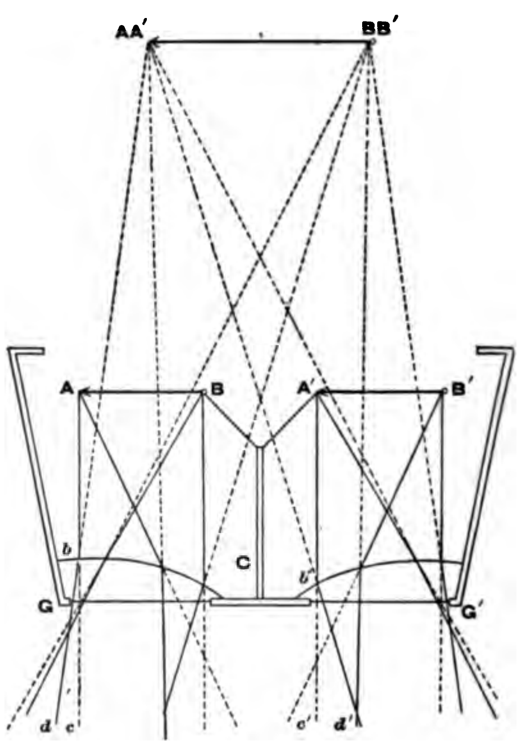


FIG. 66.

Question (5).—The accompanying diagram shows the optical principles of the instrument. *G* and *G'* are the lenticular prisms for the left and right eyes respectively. *AB* and *A'B'* are the stereograms. The ray *Ab* comes to the eye in the direction *bd*, instead of *bc*; it seems, therefore, to come from the point *AA'*. The ray *A'b'* comes to the eye in the direction *b'd'*, instead of *b'c'*; it too, therefore, seems to come from the point *AA'*. The same thing holds of rays from *B* and *B'*, and of the point *BB'*.

We accordingly see the binocular (enlarged, virtual) image at $AA'BB'$. C is the central screen. If it were absent, we should see (besides the binocular image $AA'BB'$) the figure AB with the right, and $A'B'$ with the left eye. — Ruete, Das Stereoskop, 72 ff.

The *hood* of our instrument serves to exclude lateral light. "The exclusion of all light from the eyes," writes Brewster, "and of every other light from the picture but that which illuminates it, is essentially necessary to the perfection of stereoscopic vision" (Stereoscope, 71). The *bar* allows us to adjust the instrument for eyes of different focal lengths. The *cross upright* at the end of the screen allows us to shorten the screen itself, and so to get a wide range of movement along the bar. If it were not for the upright, screens of different lengths, extending to meet the slide cards, would be required for different eyes. The *prisms*, as is plain from the diagram, bring it about that, despite the convergence of the lines of regard as one looks into the instrument, the stereograms image themselves on the retinas approximately as they would do if the lines of regard were parallel. The *lenticular* prisms have the further advantages of (*a*) enlarging the binocular image, and (*b*) serving to correct accommodation, which is normally insufficient for the close proximity of the pictures, *i.e.*, making the binocular image more distinct. — Hering, Hermann's Hdbch., 586; Helmholtz, 785; Aubert, 623.

In Brewster's own model, we have to note the following features. (*a*) The lenses are held in tubes, which move up and down, for eyes of different focal lengths. The same result is obtained in our instrument by the movement of the slide carrier along the bar. (*b*) The lenses are prevented from turning in their tubes by a pin (but see Hering, Hermann's Hdbch., 586). In our instrument the lenses are fixed, once and for all. (*c*) The lenses can be moved together and apart, for the accommodation of observers with different interocular distances. This is a great advantage. The author has found a variation in these distances of 61 to 68 mm. (*d*) Convex or concave lenses, coloured glasses, etc., can be introduced below the lenses of the instrument, for the benefit of longsighted and shortsighted observers, etc. Spectacles, carefully adjusted, answer the same purpose (Stereoscope, 66 f.). — The 'hood stereoscope,' used in our experiments, was devised by O. W. Holmes in 1861: see art. Stereoscope, by W. LeC. Stevens, in Johnson's Universal Cyclopædia.

What are the comparative merits of the two instruments? We shall get a full tale of the defects of the reflecting stereoscope if we take Brewster's criticism of it (Stereoscope, 62 f.). Brewster makes the following points. (*a*) The reflecting stereoscope is rather a clumsy and unmanageable apparatus than an instrument for general use. This is true: Wheatstone's stereoscope is, as Ruete says, an 'uncomfortable' instrument. The required solidity and ease of manipulation could be obtained by making certain parts of metal; but the cost would thereby be very greatly increased. (*b*) There is loss of light by the reflection from the mirrors. (*c*) There is a separation of the image produced by the glass surface from the more brilliant image produced by the metallic surface. (*d*) There are four refractions in each mirror, and the light is transmitted through twice the thickness of the glass. These three objections are of little weight. (*e*) The eye and all parts of the apparatus are exposed to light. This is a good point: Helmholtz, 785. (*f*) There is left-right conversion of the half slides by reflection. This is true, but the fact need not lead to any confusion. (*g*) Transparent half slides could be used only with great inconvenience, as two lights would be needed. (*h*) The size of the pictures that can be introduced is strictly limited. These two arguments are unimportant.

Wheatstone, on the other hand, declares (Phil. Trans., 1852, 5) that "there is no form of the instrument which has so many advantages for investigating the phenomena of binocular vision as the original reflecting stereoscope. Pictures of any size may be placed in it, and it admits of every kind of adjustment." This last remark touches the essential point. We have seen that in the Wheatstone instrument there is a natural conjunction of accommodation and convergence, while we can vary convergence without change of the retinal image, and vary the retinal image without change of convergence. We can also induce divergent positions of the lines of regard. There are, indeed, a number of scientific experiments that can be performed with this apparatus (Hering's haploscope is practically the same thing), but that cannot be performed with the Brewster stereoscope. Moreover, the limits of size for pictures are undoubtedly smaller in the latter instrument, unless one gives up the advantage of compendious-

ness and portability altogether; the lenses are as a rule very far from achromatism; and the plan of the instrument is less simple, even when full weight is given to the left-right reversal of the stereograms in the reflecting stereoscope. On the whole, the preference must with Wundt and Hering be accorded to the Wheatstone apparatus. The laboratory should therefore possess this, for demonstration and investigation; the refracting stereoscopes are prescribed for our experiments simply because their cheapness and compactness make it possible to procure a number of them for class purposes.

There are very many forms of stereoscope. Figs. 67, 68 and 69 show three curiosities in this line, drawn from Brewster's diagrams (The Stereoscope, 109,

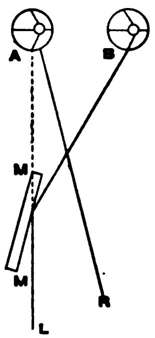


FIG. 67.

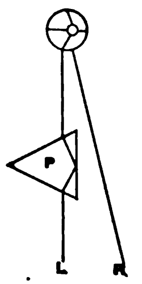


FIG. 68.

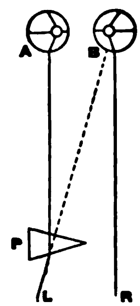


FIG. 69.

113, 119). Binocular relief is obtained in Fig. 67 by the use of two similar figures and a single mirror; in Fig. 68, by the use of a single figure and a total-reflection prism; in Fig. 69, by the use of two stereographic figures and a single small-angled prism. An instrument that deserves special mention is Helmholtz' telestereoscope, an apparatus which, as we said above, is combined with Wheatstone's stereoscope in Sanford's design. The telestereoscope exaggerates binocular relief, and is therefore particularly well adapted for bringing out the tridimensionality of very remote objects, which in ordinary vision show little or nothing of their solidity. The simpler and earlier model is given in Fig. 70. *LR* are the two eyes; *MM* two small plane mirrors, set at an angle of 90°. The sides of the instrument consist of two larger mirrors, of which the one can be turned about a vertical and the other about a horizontal axis. The course of the rays is indicated by the dotted lines above *L* and *R*. It is clear that the eyes see the binocular mirror image as if they were placed at *L'R'*, *i.e.*, at a distance very much larger than the interocular. — Helmholtz, 793 f., 822 f.

The more complicated form of the apparatus is figured by Helmholtz, 831. and is represented in schematic form in Fig. 71. — Ruete, Das Stereoskop, 83.

To use the telestereoscope, we have only to place it on the sill of an open window, or on a balcony, that commands a landscape of fair depth (it is well

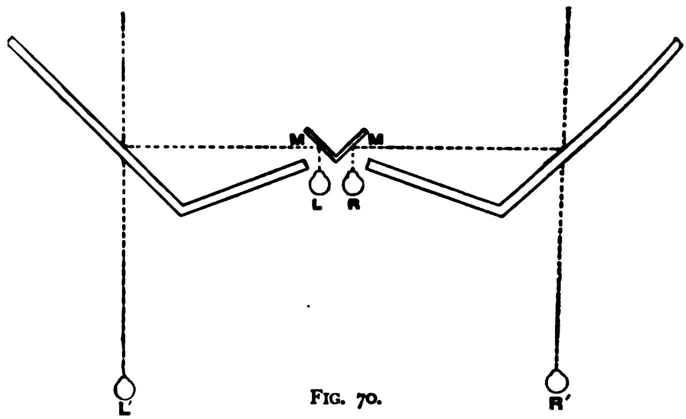


FIG. 70.

if the lines of distance in the landscape are clearly marked by rows of trees, low hills, more distant mountains, etc.), and to vary the positions of the mirrors until the monocular images combine without strain. When the principal objects in the landscape are remote and the lines of regard parallel, the binocular image has the appearance of "a very pretty and accurate model" (Helmholtz). Landscape-slides for the Brewster stereoscope, if their two views were photo-

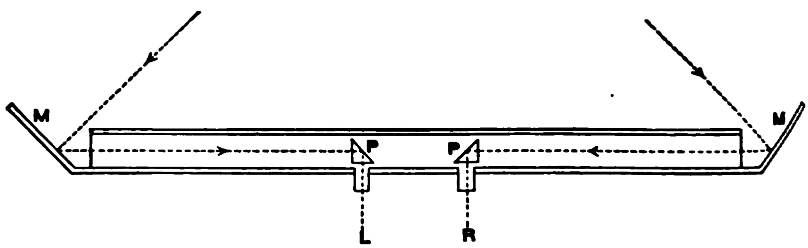


FIG. 71. — *L*, *R*, the two eyes; *P*, *P*, two totally reflecting prisms; *M*, *M*, the lateral mirrors. The dotted lines indicate the direction of the rays.

graphed from points separated by more than the interocular distance, may give the same model-effect.

Another useful laboratory instrument is the Ludwig tropostereoscope (Fig. 72). Two metal tubes 20 cm. long and 3 cm. in diameter, blackened within, are attached to a handle in such a way that the open ends can be brought together or moved apart, to match the interocular distance. The remote ends

of the tubes are supplied with threaded caps, within which coloured glass diagrams, metal discs with radii cut out, etc., can be fitted. The caps are geared together, so that the diagrams or radii can be brought into the required stereographic positions, their relief converted, etc. The tropostereoscope is evidently a refined form of the tube stereoscope referred to above, p. 260, and requires far fixation.

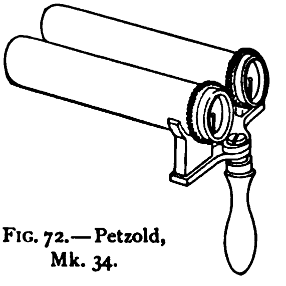


FIG. 72.—Petzold, Mk. 34.

EXPERIMENT (2). — The slides here figured should, if time permit, all be made by the student,[1] and preserved (like the kymograph tracings) either in the note-book or in a portfolio. The best published set of stereoscopic slides for the Brewster instrument is, probably, the set of 36 issued by J. Martius-Matzdorff (Die interessantesten Erscheinungen der Stereoskopie, in 36 Figuren mit erläuterndem Text, 2d edn., 1889: Winckelmann & Söhne, Berlin. The same author and publisher issue a packet of Zwölf Darstellungen des stereoskopischen Glanzes an Krystallformen, n. d.).

The selected slides are discussed by the following authors:

I., Wheatstone, 1838, Fig. 11; Ruete, 49.
II., Ruete, 48; Helmholtz, 877; Wundt, ii., 194; also Human & Animal Psych., 187.
III., Wheatstone, 1838, Fig. 12.
IV., Wheatstone, 1838, Fig. 10; Hering, Beiträge, 86; Wundt, Human & Animal Psych., 187.
V., Martius-Matzdorff, no. 19.
VI., Martius-Matzdorff, no. 20; Wundt, ii., 180.
VII., Martius-Matzdorff, no. 22; Wundt, ii., 182; Human & Animal Psych., 185.
VIII., Martius-Matzdorff, no. 23.
IX., Martius-Matzdorff, no. 24.
X., Martius-Matzdorff, no. 26.
XI., Martius-Matzdorff, no. 27.
XII., Martius-Matzdorff, no. 29.
XIII., Martius-Matzdorff, no. 32.
XIV., Le Conte, Sight, 137.
XV., in the Cornell Laboratory series, is a photograph of the interior of the railway bridge at Cologne.

[1] If time does not permit of this, the student should at least make slides i., ii., iv., xviii., xix., xxi., xxv., xxviii., xxx., xxxv. and the colour slides.

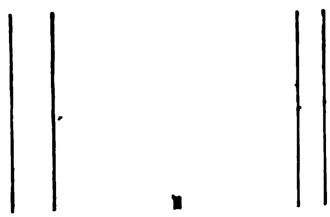
XVI. is a lunar photograph by Warren de la Rue, London.
XVII. is a photograph of ruins of temple, Sphinx and Great Pyramid, published by Strohmeyer & Wyman, New York. (These three slides chance to be the best of the Cornell collection for the purposes of this experiment. Every laboratory will, doubtless, possess a large number that will answer the purpose more or less completely.)
XVIII., Hering, Beiträge, 84.
XIX., Hering, Beiträge, 85.
XX., Hering, Beiträge, 86.
XXI., Helmholtz, 881; Wundt, ii., 193; Human & Animal Psych., 187.
XXII., Wheatstone, 1838, 385, Fig. 24; Helmholtz, 882; Wundt, ii., 193; Sanford, Lab. Course, 291.
XXIII., Martius-Matzdorff, no. 13.
XXIV., Wundt, ii., 193; Sanford, Lab. Course, 292.
XXV., Hering, Hermann's Hdbch., 380; *cf.* Helmholtz, 917.
XXVI., Wheatstone, 1838, Fig. 25; Wundt, Human & Animal Psych., 198.
XXVII., Hering, Hermann's Hdbch., 383; Helmholtz, 918.
XXVIII., Hering, Hermann's Hdbch., 382.
XXIX., Wundt, ii., 211; Human & Animal Psych., 198; Helmholtz, 919.
XXX., Hering, Hermann's Hdbch., 383; Helmholtz, 919.
XXXI., Halves of Martius-Matzdorff, nos. 28, 30.
XXXII., Martius-Matzdorff, no. II. of set of lustre slides; Helmholtz, 933; Sanford, 173.
XXXIII., Wundt, ii., 209.
XXXIV., Wundt, ii., 210.
XXXV., Wundt, ii., 210.
XXXVI., Wundt, ii., 210.
XXXVII., in the Cornell series, is a photograph entitled "Where the water-lilies bloom in March; Florida" (child in boat, among water-lilies), and published by The Littleton View Co. It shows patches of lustre, as well as true reflections.

Slide i. The four dots combine, and we see two, of which the right is nearer to, the left farther from, the observing eyes.

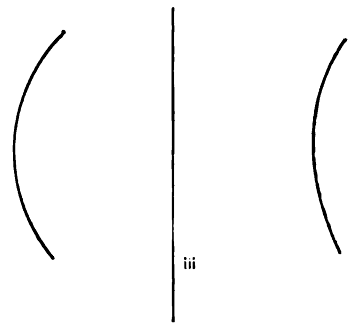
SLIDE I.

EXPERIMENT (3). — Slide ii. The lines combine, with the same effect as is given by the dots. Slide iii. The curves combine, the concavity being towards the observer.

Inversion of i. and ii. gives dots and lines of which the right-hand member is farther off. Inversion of iii. (and here the un-

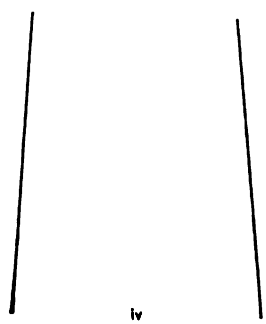


SLIDE II. — Full size.

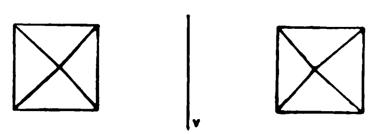


SLIDE III. — Full size.

wary student is apt to predict falsely) gives concavity, again, only that the left-hand direction of the curved line is reversed.

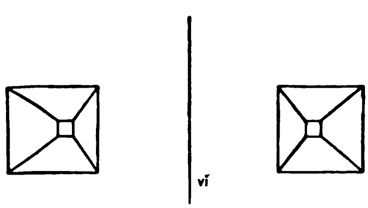


SLIDE IV. — Full size.

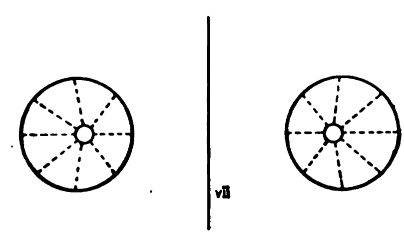


SLIDE V. — Half size.

A neat variation of Slide iii. is a slide composed of the two right-hand (or left-hand) halves of the truncated-cone stereo-

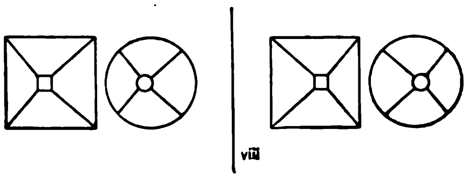


SLIDE VI. — Half size.



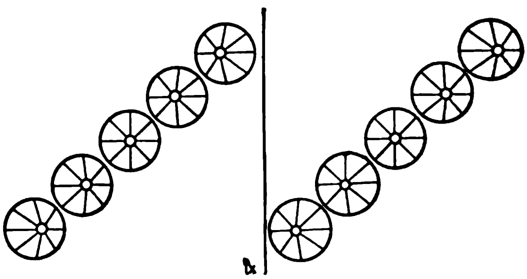
SLIDE VII. — Half size.

grams. If, *e.g.*, halves of the figures of Slide vii. are taken, their combination shows two curves, of which the smaller is the



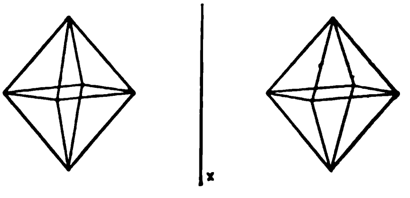
SLIDE VIII. — Half size.

nearer. Ask what will happen when the slide is inverted, and a good part of the class will reply at once that the cone in relief will be converted into a hollow cone.



SLIDE IX. — Half size.

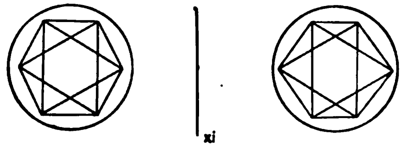
EXPERIMENT (4). — Some students will probably draw the double images as they look when both eyes are open, instead of drawing the single image of each eye separately. The result is



SLIDE X. — Half size.

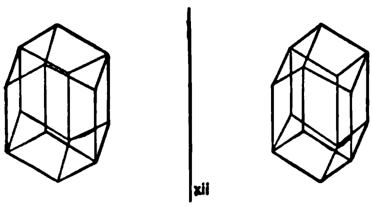
a reversal of direction. Note that Wheatstone and Hering draw the diagram correctly (Wheatstone for the reflecting stereoscope, Hering for crossing of the lines of regard before the

plane of the page), while Wundt has interchanged the explanations of his two Figs., 28 and 29 (H. and A. Psych., 187).

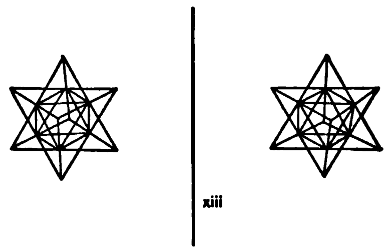


SLIDE XI. — Half size.

EXPERIMENT (5). — The model-effect, referred to on p. 272, is very striking in lunar photographs. Notice that, for a careful

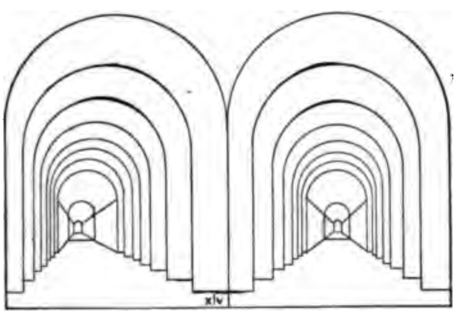


SLIDE XII. — Half size.



SLIDE XIII. — Half size.

observer, the combination of the geometrical figures is rarely complete. If the base of the object is fixated, the vertex falls

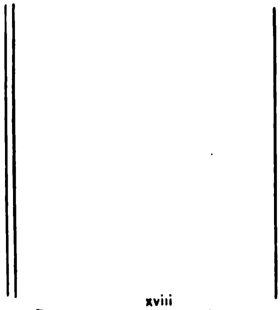


SLIDE XIV. — Half size.

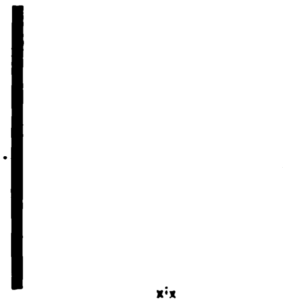
into double images; and, conversely, if the vertex is fixated, the base is seen double. If a point is taken midway between base and vertex, the binocular image at first appears single, but con-

tinued steady fixation may bring out double images both before and behind the point of regard.

Experiment (6).— Slide xviii. gives the right and left views of two lines, situated in a vertical plane passing through the



Slide xviii. — Full size.



Slide xix. — Full size.

direction of regard of the right eye. The combined image verifies this analysis. Vary the slide, by increasing the distance between the left-hand parallels. Draw the figures, with increased distance between the parallels, upon a transparent slide. See Hering, Beiträge, 84.

Slide xix. gives the right and left views of a flat ruler, standing in the vertical plane which contains the line of regard of the right eye.

Slide xx. gives the two projections of an obtuse angle, whose limbs diverge from the observer over against the left eye. Vary the slide, by left-right conversion of the right-hand figure, and the obtuse angle opens towards you, over against the same eye.

Slide xx. — Full size.

Fig. 73.

Fig. 73, which is known as 'Wheatstone's Figure,' has played a large part in the discussions of stereoscopic vision (Wheatstone, 1838, 384 f.). Its final and complete analysis has been given by Hering (Beiträge, 87–96). The

working-through of Hering's demonstration with free stereoscopy (convergent squinting) is good practice, and the experiment is theoretically important. Wundt's account (ii., 195; Beiträge zur Theorie d. Sinneswahrnehmung, iv., 1862, 286; Human and Animal Psych., 191) is incorrect, or at least inadequate. In this additional experiment, Hering's caution as to the horizontal lines of separation must be borne in mind (Beiträge, 89). The parallel lines of the figures must remain sensibly parallel until the moment of fusion.

Slide xxi. — Full size.

Experiment (7). — The combined image of Slide xxi. shows simply two horizontal lines in the plane of the slide-card. Combination is more difficult than in the case of Slide ii.; and a steady fixation of the lines, drawn as above, may bring out the double images. The discrepancy may be very much larger in the case of the vertical lines, though there are great individual differences in this regard (Wundt, ii., 104).

The two circles of Slide xxii. combine; but careful observation shows that, while the combination is perfect at the sides

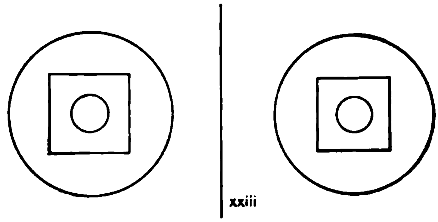
Slide xxii. — Full size.

(vertically), it is constantly lapsing above and below (horizontally). *Cf.* Helmholtz, 882 f.

Slides xxi. and xxii. might represent a single object situated close up to the eyes on the extreme left of the observer; the image on the right retina would, in such a case of asymmetrical convergence, be smaller than that on the left. Wheatstone, 1838, 386.

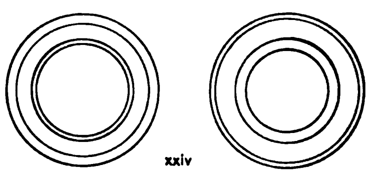
Slides xxiii. and xxiv. cannot represent a single object. In Slide xxiii., the diameters of the small circles should differ by .5 mm., the sides of the squares by 1 mm., and the diameters of the large circles by 2 mm. In Slide xxiv., the second and outer

circles are alike, the central and third circles different. Notice the readiness of combination, as one first looks at the pictures in the instrument, and the consequent lapse into double images above and below under steady fixation.



SLIDE XXIII. — Half size.

The general lesson is that "we combine in a single idea retinal images that cannot possibly proceed in reality from a single object, provided only that they approximate very closely to the real images of an object" (Wundt). The greater our practice in the observation of double images, the more difficult is it for us to secure a true binocular fusion. This is true, not



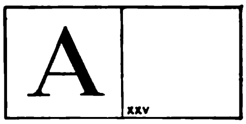
SLIDE XXIV. — Half size.

only of diagrams like those of Slides xxiii. and xxiv., but of stereograms in general (see Hering, Beiträge, 109, 337; Hermann's Hdbch., 432).

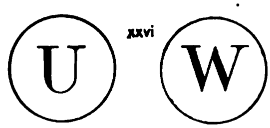
EXPERIMENT (8). — We might expect that the *A* of Slide xxv. would be washed over by the white background of the right monocular field, so that it would appear greyish and indistinct. In actual fact, the letter is seen as clearly as if it combined with another *A* on the other half of the slide.

In Slide xxvi. we have no combination in a single idea. We may see either letter alone, or fragments of the two letters simultaneously. In neither case is there any permanence of the

binocular impression; on the contrary, there is a constant building-up and breaking-down of images. The variation called for in the text consists in the attempt to unite two letters whose forms are partially identical, such as *E* and *F*, *L* and *F*, *C* and *G*,



SLIDE XXV. — Half size.



SLIDE XXVI. — Half size.

O and *Q*, *P* and *B*. The resultant letter is not quite so steady as the *A* of Slide xxv.

In Slide xxvii. we may see the vertical band continuous, with a lustrous greyish fringe to right and left of the crossing-point; or the horizontal band continuous, with the fringe above and below; or a black central square, with grey fringes above, below

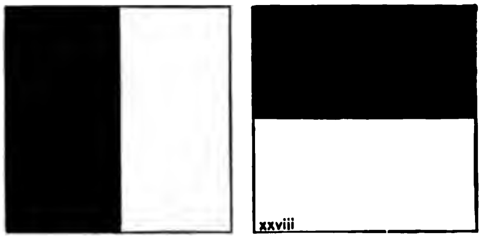
SLIDE XXVII. — Full size.

and to right and left. We never see a continuous cross, such as we should get if the two bands imaged themselves upon the same retina.

In Slide xxviii. we see a square, whose left upper quadrant is black, the right lower quadrant white, while the other two quadrants are (over most of their surface) a lustrous grey. Note the permanent contrast bands, and the rivalry of the vertical and horizontal contours.

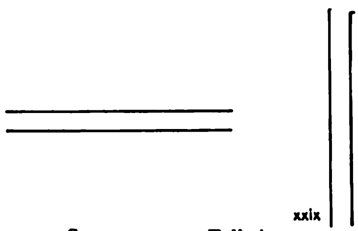
In Slide xxix. we see a total image, in which the lines in the one direction are interrupted by those in the other. The interruption oscillates, from the vertical to the horizontal lines or

vice versa. There may be, indeed, not only an interruption, but an actual suppression, so that the portions of the one pair of



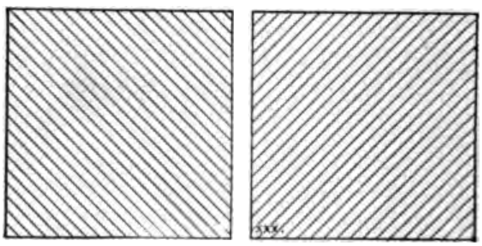
SLIDE XXVIII. — Full size.

lines that should appear between the other pair are entirely obliterated; or there may be unilateral suppression, one line of



SLIDE XXIX. — Full size.

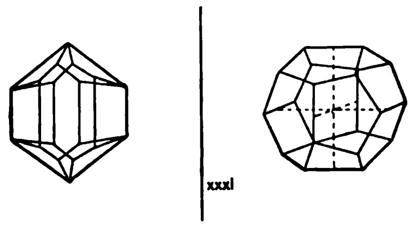
a pair showing a gap in its middle, while the other continues its full course uninterrupted save by the crossing contours of the



SLIDE XXX. — Full size.

other pair. We have, then, in this slide, a suggestion of depth, since the one pair of lines may seem to lie behind the other pair; and we have, further, the rivalry of contours. Slide xxx. shows

a similar phenomenon. Slide xxxi. is extremely baffling. One has a very distinct suggestion of tridimensionality, and the eyes soon grow fatigued in the attempt to 'set themselves' for a true binocular combination. The combined figure becomes flatter and flatter under steady fixation. The rivalry of contours may be noticed at the points of crossing of the boundary lines.



SLIDE XXXI. — Half size.

The phenomena to which this set of slides introduces us are those of the prevalence of contours, the rivalry of contours and lustre. (1) Slide xxv. gives a pure instance of the prevalence of contours. The white of the right-hand field is 'suppressed' by the *A* of the left-hand field. This fact may be generalised as follows: "Any contour in the one retinal image assists the adjacent portions of the field to a permanent victory over the differently tinted ground of the other retinal image" (Hering). (2) Slides xxvi.–xxxi. show the rivalry of contours. "If contours in the two retinal images take such directions that there is retinal congruence for only a single point of each, they appear to cross one another at the point of the visual field which corresponds to this pair of congruent points, but always in such a manner that the one contour (together with the strip of ground adjacent to it) interrupts the other or is interrupted by it" (Hering). Hering finds the significance of the prevalence and rivalry of contours in the fact that "they prevent the fusion of the two retinal images, and secure to each a certain amount of independence." Without them, the images of the two unmoved eyes would necessarily run together, as objects directly seen through an unmoved glass and objects mirrored in it run together. They are, indeed, a *sine qua non* of the binocular per-

ception of depth. Nevertheless, Hering does not attempt any detailed explanation of the facts. He recommends the "treffliche Schrift" of P. L. Panum (Physiologische Untersuchungen über das Sehen mit zwei Augen, Kiel, 1858); and, as Panum's explanation is physiological (p. 47), and Hering himself inclines always to physiological as distinct from psychological interpretations, we may take it for granted that the explanation would be couched in strictly physiological terms (Beiträge, 308 ff., 312 ff.; Hermann's Hdbch., 384 f.; *cf.* Helmholtz, Phys. Optik, 922; Sanford, Lab. Course, 171). Helmholtz, on the other hand, argues from the phenomena of rivalry that "the contents of each visual field comes to consciousness separately, without being fused with that of the other by means of some physiological mechanism," and that "the fusion of the two fields in a common image, if it occurs, is consequently a psychical act." His own explanation is couched in terms of attention (922 ff.). Fechner gives a full discussion of the attention theory in his paper Ueber einige Verhältnisse des binocularen Sehens (Abh. d. kgl. sächs. Ges. d. Wiss., vii., 1860, 392 ff.). His conclusion is that the attention may occasion a change of the image seen, but can never determine the direction of the change (402). Wundt deduces from the phenomena of binocular mixture, suppression, rivalry and lustre the general law that "the impressions of the two eyes always fuse to a single idea" (ii., 214). Where reference to a single object is impossible, we have mirroring and lustre, or rivalry; but there is always a fusion, a single resultant. Rivalry itself is determined by eye movements: "that image is always preferred, whose contours run in the same direction as the (accidental or purposed) movement of regard" (213; *cf.* Beiträge zur Theorie d. Sinneswahrnehmung, 362; Human and Animal Psych., 209).

(3) Slides xxvii. and xxviii. show the phenomenon of lustre, to the consideration of which we now pass.

Experiment (9). — If a black and a white fall upon the same portion of a single retina, we see an intermediate grey. If a black and a white fall upon corresponding portions of the two retinas, we see, not a grey (*cf.* Slides xxvii., xxviii.), but a graphite-like lustre or sheen. To understand this effect, we

must understand the psychology of reflexion, of the perception of mirror images.

Wundt's Experiment. — Lay a square of red paper, *a*, Fig. 74, upon a grey background. Above it, at an angle of 45°, set up the sheet of glass *g*. Lay a square of white paper, *b*, upon a similar grey background, in the position indicated in the figure. The eye, looking through *g* at *a*, sees the white image of *b* (marked *b'* in the figure) mirrored behind the red of *a*. Neither the red nor the white has suffered any loss of individuality; neither, *i.e.*, has taken on any tinge of pink.

If the grey ground of *b* is moved to the position *c*, there is no reflexion, but simply mixture; we see a single pink square. If

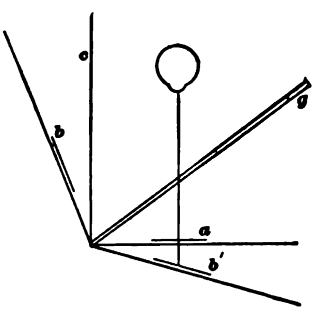


FIG. 74.

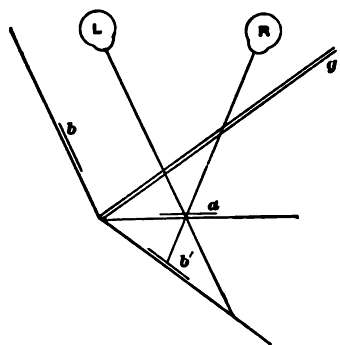


FIG. 75.

the grounds are left as before, but are themselves coloured red and white, we again get no reflexion, but a single pink surface. If, finally, we draw small outline squares in black upon these red and white grounds, the phenomenon of reflexion reappears; each square is assigned to its appropriate distance.

Now give the eyes and the apparatus the positions indicated in Fig. 75. The left eye sees *a* alone; the right eye sees the image *b'* mirrored behind *a*. If *b* is very bright, and if *b'* covers the whole of *a*, the latter may be completely ignored: the left eye then sees *a*, and the right eye sees only *b'*. Under these conditions we have, as we had at first, the single idea of a reflecting object, and a clear discrimination of the reflecting surface from the image mirrored behind it. — Wundt uses this latter

result to explain the suppression of the middle portion of the lines of Slide xxix. "Where the position of the object [the reflecting surface] corresponds to that of the reflected image, the object [the reflecting surface] is ignored; just as those portions of one of the stereoscopic pictures which were covered by lines of the other picture were ignored." The uninterrupted lines are seen *through* the interrupted lines (Human and Animal Psych., 202; Phys. Psych., ii., 214). Does this interpretation agree with *O*'s introspection?

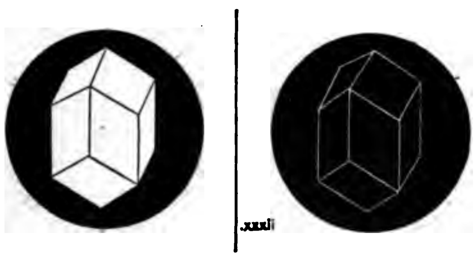
The conditions under which this perception of transparency, of the seeing of an object behind another object, arises are stated by Wundt as follows. "We say that a surface reflects, or is transparent, when it gives perfectly clear mirror-images, while there is still some sort of indication to remind us of its own presence. A few patches of brighter illumination, which are therefore lustrous, would serve this end." Note that the reflector and the reflected image are never seen in strict simultaneity, since the field of vision for Wundt is always a surface. If the mirroring is perfect, we lose the reflector, except that its contours may serve to 'frame' the reflected object; if it is imperfect, we get the single-surface perception of lustre. For "we say that a surface is lustrous, when the mirror-image that it gives is very indistinct; when a clear apprehension of the mirror-image is prevented by irregularities of the reflecting surface; or (and this is the commonest case) when both these factors are coöperating to produce the result" (Phys. Psych., ii., 205; *cf.* the original discussion in the Beiträge zur Theorie der Sinneswahrnehmung, 300 ff., and Aubert, Physiol. d. Netzhaut, 1865, 302 ff.). Mirroring and lustre are, then, our perceptual 'way out' of the difficulty of making two heterogeneous groups of sensations into a single idea. Wundt's analysis is structural; his explanation is purely functional.

Helmholtz' account is very similar. If light falls upon a dead-finished surface, he says, it is reflected in all directions in such a way that the surface appears uniformly bright from whatever point it is viewed. Lustrous surfaces are those that give more or less regular mirror-reflexions. Suppose that the surface is smooth, in all its parts, but is not quite even: then, as we

look at it, one of our eyes may be in the direction of the reflected light, and the other not. The surface will thus seem to be of a different brightness to the two eyes. If, therefore, we synthetise the conditions, and offer black to the one eye in the stereoscope, and white to the other; or if we offer one colour to the one eye, and another colour to the other, —choosing such colours as a really lustrous surface might present; we necessarily obtain the effect of lustre from our combined image (933 f.; *cf.* Sanford, Lab. Course, 173).

Hering, who also refers lustre to irregular or imperfect reflexion, writes that the condition of its appearance is "a cleavage of sensation; a portion of the sensation seems to be the essential colour of the surface, while other portions are regarded as accidental light or shade, lying on or before the surface, or proceeding from the interior of the lustrous body." He notes that rivalry of the monocular fields is favourable to lustre, since the required cleavage of the total sensation is directly given. Movement, whether of the lustrous object or of our own bodies (*e.g.*, change of ocular convergence), is also a favourable condition (Hermann's Hdbch., 576 f.).

We may now consider the slides of this group. — Slide xxxii. shows a graphite lustre. This was discovered by H. W. Dove

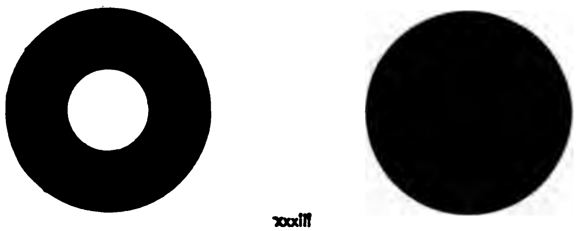


SLIDE XXXII. — Half size.

(see Darstellung der Farbenlehre und Optische Studien, Berlin, 1853, 171 [the original paper was published in 1851]; Optische Studien, Fortsetzung, 1859, 1 ff.).

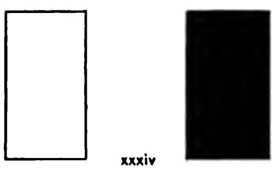
Slide xxxiii. shows no lustre. The white disc appears, without darkening or sheen, in the middle of the binocular image. The slide is, therefore, analogous to Slide xxv. rather than to

Slide xxxii. If lustre or mirroring is to arise, two conditions must be fulfilled: (1) the impressions must be so far different that they can be referred to two different objects, a reflecting

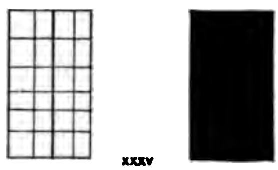


SLIDE XXXIII. — Full size.

and a reflected; and (2) they must force themselves upon perception with approximately equal intensities. This latter condition is not satisfied by Slide xxxiii.



SLIDE XXXIV. — Half size.



SLIDE XXXV. — Half size.

Slide xxxiv. shows lustre. The slide is best made by pasting strips of black and white paper upon a grey ground. Slide xxxv. gives the same effect as Slide xxxiii., and for similar reasons.

SLIDE XXXVI. — Full size.

In Slide xxxvi. the smaller disc, together with its immediate surroundings of white, seems to lie behind the large disc. Lustre is seen, but not so plainly as in Slide xxxiv.

Slide xxxvii. may be replaced by any photographic slide showing polished tables, columns, etc.; rippling water, with sunlight upon it; satin dresses or hangings; plants with lustrous leaves, etc. (Helmholtz, 933). Some wet stones in A Mirror View of the Forum, published by J. F. Jarvis, give a very striking lustre. Notice that the beginner may mistake the photographic glaze for the lustre of the pictured objects.

Experiment (10). — The essential point in this experiment is the production of a binocular colour mixture. The possibility of binocular mixtures has been keenly disputed. "Hermann Meyer, Volkmann, Meissner, Funke and I myself," says Helmholtz, "have never seen the mixed colour; Dove, Regnault, Brücke, Ludwig, Hering and Panum declare that they are able to see it" (Phys. Optik, 926). Wheatstone (1838, 386 f.) might have been added to the list of negatives. Helmholtz ascribes the illusion of binocular mixture to various conditions: lack of check or control of the experiment by simultaneous vision of the true (monocular) mixed colour, and consequent failure to perceive the rivalry which is really present; after-images; colour induction; contrast. He admits, however, that there may be great individual differences as between different observers. Hering asserts that the discrepancy of result is due, quite apart from individual differences, to diversity of the conditions of observation, and to divergent interpretations of the term 'binocular mixture' and correspondingly divergent expectations as to the character of the combined image (Hermann's Hdbch., 592). The one thing needful for binocular mixture-effects is the elimination of contours, points and, indeed, any sort of irregularity, from the coloured surfaces. Helmholtz neglected this precaution, and was further led astray by his presupposition that the result of binocular would be identical with the result of monocular colour mixture (595, 599). This is not the case. The facts are summed up by Hering in his general law of the "complementary share of the two retinas in the visual field" (Beiträge, 308 ff.; Hermann's Hdbch., 596 ff.). There is no addition of the monocular sensations: the resultant sensation is always $= 1$. Hence, if the one retina furnish $\frac{3}{4}$ of this resultant, the other must furnish $\frac{1}{4}$; if the one furnish $\frac{1}{2}$, the other must also furnish $\frac{1}{2}$; if

the one furnish 1, the other must furnish 0. — The working-out of this law in binocular and monocular mixtures, and in certain of Fechner's binocular experiments, is very instructive; if time allow, it may be given to the student as an extra experiment.

Slide xxxviii. and its Variants. — The author has no doubt but that binocular mixture occurs, and no doubt but that some of these slides will demonstrate its occurrence to every student who observes the conditions of the experiment. The squares must be so placed as to be entirely and exactly coincident in the binocular field, and it is well to throw them a little out of focus (*i.e.*, to look at them with inadequate accommodation), in order to blur the contours. Note (1) that neighbouring colour-tones give the mixture, as a rule, more easily than complementary colours. It is, however, possible to combine complementaries to a binocular grey. (2) The less the saturation, the easier as a rule is the binocular mixture. It is, however, possible to combine saturated colours. (3) The less the brightness, and the more nearly equal the brightness of the combined colours, the easier is the mixture. Bright colours can, however, be combined.

In the cases where mixture is impossible, the observed phenomena will be those of retinal rivalry. Now the one, and now the other colour will be seen; now the one will seem to hang, like a translucent veil, before the other; now a patch of the one will give way to the other, which spreads gradually over the whole square; now the two will give a brilliant lustre. The changes should be carefully noted by *O*, and their times taken by *E*.

Slide xxxix. — The two extreme squares are monocular images, and are unimportant for the present purpose. The three middle squares are binocular images: the two outer show the pure colour, the middlemost shows the mixture colour. See Hering, Hermann's Hdbch., 592. The student should draw a diagram, indicating how the images arise.

Slide xl. — At the centre, where green and red meet, we see simply these two colours; toward the outside they are intermixed with a bluish colour. The slide is, therefore, analogous to Slide xxxvi.

Slide xli. — We see, upon the red background, a large blue square, in the middle of which is a small yellow square surrounded by a fringe that shows a deep red on the inside, but becomes more and more tinged with blue towards the outside. Wundt (Human and Animal Psych., 208) explains the result entirely in terms of reflexion. Can it be explained in terms of dominance of contours?

These colour-slides may be varied indefinitely, as further questions suggest themselves. It is of advantage, in some instances, to put aside the stereoscope and have recourse to free stereoscopy. It is still better to use Hering's binocular colour mixer, although a successful handling of the instrument requires a certain amount of skill and practice. A combination of such work with a careful repetition of the experiments cited by Helmholtz against binocular mixtures in general forms an excellent additional experiment. In any case, the interested student should be allowed critically to repeat Helmholtz' experiments.

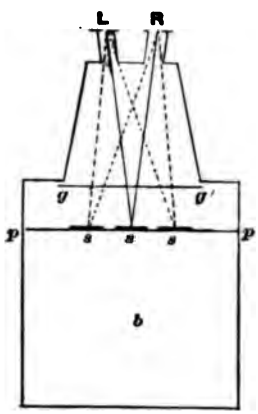


Fig. 76. — Hering's binocular colour mixer (Rothe, Mk. 32). L, R, the two eyes; *b*, dark box; *gg'*, coloured glasses (red and blue); *pp*, supporting plate of clear glass; *sss*, squares of white paper. Hermann's Hdbch. d. Physiol., iii., 1, 1879, 593.

Question (6). — The terms and phrases are: prevalence of contours, suppression, rivalry of contours, retinal rivalry, lustre, binocular mixture, reflexion or mirroring. These have been sufficiently explained in the foregoing discussions. The attention of the student may be called (if he has not discovered the fact for himself) to the part played by monocular contrast in some of the slides where its presence has not been expressly noted.

(7) Cover the one half of a slide with black, the other with white paper. On the first half, paste a small square of white, on the second a similar and congruently placed square of black. Notice that there is rivalry between backgrounds and squares alike. This is said by Hering (Beiträge, 309) to prove the point.

(8) The slide should be made on the analogy of Slide viii. Draw, side by side, the stereograms of two precisely equal and similar truncated pyramids: but draw the one for a solid and

the other for a hollow effect. Notice that the square that comes out towards you looks distinctly smaller than the square that bounds the far end of the hollow pyramid.

Related Questions. — (9) One-eyed persons have no difficulty in finding their way about; and we, ourselves, if we close one eye, suffer from no illusion as to the solidity of the objects around us. It must be remembered, however, that in such cases (1) the observer can change his position with regard to surrounding objects; (2) the objects themselves may change their positions, with regard to him and to one another; and (3) a number of secondary criteria of distance are still available. How is it if these auxiliary factors are ruled out?

There is a 'parlour game' which bears upon this point. A curtain ring is suspended in the median plane of the observer's body. He is given a pencil, and required to thrust the pencil through the ring, with one eye closed. The pencil passes at 'surprising' distances before or behind the ring. Plainly, then, binocular vision is required for accurate localisation. — See, for a better form of the experiment, Helmholtz, 796; Sanford, exp. 216. *Cf.* Hering, Beiträge, 347; Hermann's Hdbch., 391. The 'game' may be raised to the dignity of an experiment by a careful ruling-out of possible secondary factors: thus the observer may look through a blackened tube, so that he does not see the hand that holds the pencil; rings and threads of different diameters may be used interchangeably, etc. See Höfler and Witasek, Psychologische Schulversuche, 1900, 21.

Our particular Question may be answered, roughly, by Brewster's statement (Stereoscope, 3) that monocular stereoscopy is possible only with pictures, not with diagrams. This is so far true as that the secondary criteria of the depth-perception are of enormous advantage in monocular stereoscopy. It is, however, not strictly true. There are diagrams, as Hering says (Beiträge, 66, 78 f.), which "incite or even constrain us to the perception of depth," in the absence of the secondary factors.

Place Slide vi. or vii. in the stereoscope. Close one eye, and look at the half-stereogram with the other. In all probability, you will obtain the image of a cameo or intaglio, the figure

oscillating from the one form to the other (see p. 310). The relief is not nearly so well marked as it is when both eyes are open: test this, by opening the closed eye at a moment when the relief is clearly seen in monocular vision. Steady fixation of the centre of the figure enables one to keep the image in the plane of the card for some little time together.

Vary the experiment, by looking into the stereoscope, first, with both eyes open, and then closing one eye. In most cases, there is an immediate conversion of relief, which is the more pronounced the less practised the observer. In a few seconds, the figure comes to the plane of the card, and the oscillation of cameo and intaglio begins.

Place Slide xv., xvi., or xvii. in the stereoscope. Notice that Slides xv. and xvii. give a good stereoscopic effect in monocular vision (allowance must be made for the difference in brightness between this and binocular vision!), whereas Slide xvi. shows hardly any relief at all. See Aubert, Physiol. d. Netzhaut, 323 ff.; Helmholtz, 767 ff.; Wundt, ii., 204; Wheatstone, 1838, 380.

(10) Brewster, speaking of diorama, says: "The light, concealed from the observer, is introduced in an oblique direction; and the distance of the picture is such that the convergency of the optic axes loses much of its distance-giving power. The illusion is very perfect, especially when aided by correct geometrical and aerial perspective." "If light come from various directions, or the canvas move to the least degree, the illusion is gone" (Stereoscope, 2 f.). In the more modern cyclorama, one has constancy of illumination; distance; correct perspective (the technique of such painting has improved very greatly since Brewster wrote); a 'real' foreground, blended skilfully with the scenes of the painted wall; and illusory surroundings (one is on the roof of a house, or on a hillock, in the midst of the scene portrayed). See Aubert, Physiol. d. Netzhaut, 324; Phys. Optik, 619 f. (Aubert notes that vision of the painted surface through a large convex glass renders our estimate of its distance uncertain, and so enhances the illusion); Höfler, Psychologie, 294; Helmholtz, 776.

(11) The secondary criteria may be summed up as follows. (*a*) Linear perspective; the course of the contour-lines of ob-

jects in the field of vision. (*b*) Aerial perspective. This may be generalised as relative clearness of outline and colour-tone. (*c*) Distribution of light and shade. (*d*) Interposition; the partial covering of far by nearer objects. (*e*) Especially in the case of familiar objects, apparent magnitude (visual angle). (*f*) Movement of objects in the field of vision. (*g*) Movement of our own head or body. If we fixate a near object, and move the head to one side, distant objects show a movement in the same direction; if we fixate a far object, and move the head as before, nearer objects show a movement in the opposite direction. — Hering, Hermann's Hdbch., 578 ff.; Wundt, ii., 199 ff.; Helmholtz and Aubert, as quoted; Brewster, Stereoscope, 44 f.; Titchener, Outline, 204 f.; Sanford, Lab. Course, exps. 176, 183, 184, 188.

(12) Brewster assigns a triple superiority to monocular vision. (*a*) Reflected light is shut off, so that there is less suggestion of a plane surface; (*b*) there is no 'convergency of the optic axes' to indicate a plane surface [the student should perform Donders' experiment; Aubert, Phys. Optik, 620]; (*c*) possible differences between the two eyes are eliminated. — Stereoscope, 45 f. *Cf.* Aubert, Phys. d. Netzhaut, 324; Wheatstone, 1838, 380 f.; Wundt, ii., 203.

LITERATURE:

Sir D. Brewster: The Stereoscope, its History, Theory and Construction. London, 1856.

J. Le Conte: Sight, an Exposition of the Principles of Monocular and Binocular Vision. International Scientific Series, 1881.

W. N. Suter: Handbook of Optics for Students of Ophthalmology. New York, 1899.

R. T. Glazebrook: Light, an Elementary Text-book, theoretical and practical. 2d edn. Cambridge, 1895.

C. G. Th. Ruete: Das Stereoskop, eine populäre Darstellung mit zahlreichen erläuternden Holzschnitten und mit 27 stereoskopischen Bildern. 2d edn. Leipzig, 1867.

H. W. Dove: Darstellung der Farbenlehre und Optische Studien. Berlin, 1853.

H. W. Dove: Optische Studien, Fortsetzung der in der 'Darstellung der Farbenlehre' enthaltenen. Berlin, 1859.

C. Wheatstone: Contributions to the Physiology of Vision. i. On some remarkable, and hitherto unobserved, Phenomena of Binocular Vision. Phil. Trans. of the Royal Society of London, 1838, Pt. ii., 371 ff.

C. Wheatstone: Same, ii. Phil. Trans. of the Royal Society of London, 1852, Pt. i., 1 ff.

H. von Helmholtz: Handbuch der physiologischen Optik. 2d edn. Hamburg and Leipzig, 1896.

E. Hering: Der Raumsinn und die Bewegungen des Auges. In Hermann's Handbuch der Physiologie, iii., 1, 343 ff. Leipzig, 1879.

EXPERIMENT XXVIII

§ 48. **The Pseudoscope.** — The total-reflexion pseudoscope was figured and described as such by Wheatstone in 1852 (Phil. Trans., 10 ff.). Fig. 77 shows its original form. A year before, H. W. Dove had invented the same instrument under the name of the 'prism stereoscope.' Dove was desirous of constructing a stereoscope which should be free both from the secondary mirror-images (reflexions

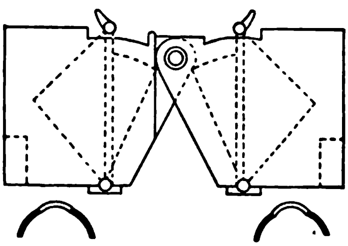


FIG. 77. — Wheatstone's total-reflexion pseudoscope.

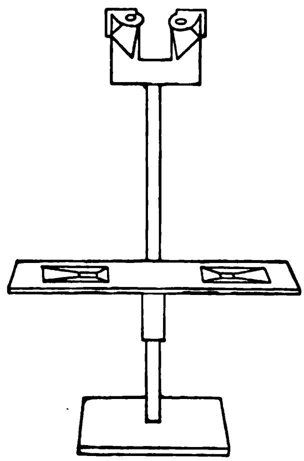


FIG. 78. — Dove's converting stereoscope.

from the mirror-surfaces) of the Wheatstone stereoscope, and from the chromatic defects of Brewster's semi-lenses. His choice was thus narrowed down to metallic mirrors and total-reflexion prisms, and he selected the latter (Farbenlehre und Optische Studien, edn. of 1853, 194 f.). Fig. 78 shows one of Dove's instruments.

QUESTIONS. — (1) Fig. 79 indicates the optical principles upon which the total-reflexion pseudoscope is based. *O* is the object viewed; *DD* the pseudoscopic double images; *re* the rays coming to the eyes. As the prisms are turned, and the lines of

regard correspondingly converged, the double images overlap to form a binocular total image, while the object of course disappears. *Cf.* Figs. 58, 59, 60 above. — Ruete, Das Stereoskop, 84; Sanford, Lab. Course, exp. 214.

The tubes, in the instrument recommended in the text, serve the purpose of the hood of the stereoscope; the turning of the tubes about the vertical axes answers the same purpose as movement of the slide carrier along the bar of the stereoscope; and the to and fro movement of the left-eye tube allows the instrument to be adjusted for different interocular distances.

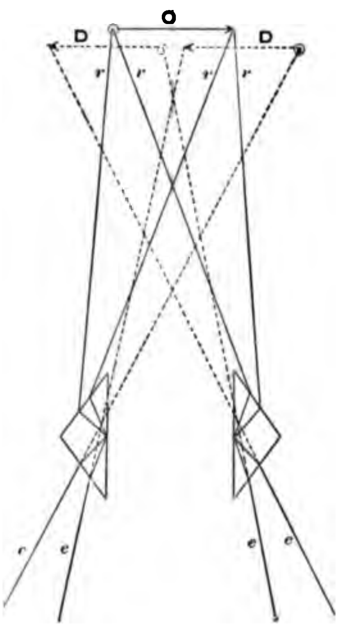


Fig. 79.

"Each eye," says Wheatstone, "will see [in the pseudoscope] a reflected image of that projection of the object which would be seen by the same eye without the pseudoscope" (12). The conversion is, therefore, that of the second line of Fig. 25 of the text.

Some other forms of pseudoscope may be mentioned here. (1) *Mirror pseudoscopes.* (*a*) Wheatstone describes a mirror pseudoscope of his own devising as follows. "Two plane mirrors are placed together so as to form a very obtuse angle towards the eye of the observer; immediately before them the object is to be placed at such distance that a reflected image shall appear in each mirror. The eyes being placed before and a little above the object, must be caused to converge to a point between the object and the mirrors; the right-hand image of the left eye will then unite with the left-hand image of the right eye, and the converse relief will be perceived. The disadvantages of this method are that only particular objects can be examined, and it requires a painful adaptation of the eye to distinct vision" (16). The stages of conversion are: $a - b' - b$; see answer to Question (4), below.

(*b*) J. R. Ewald's pseudoscope is represented diagrammatically in Fig. 80. The rays proceeding from the object fall upon the mirrors *MM*, and are twice reflected before reaching the eyes. The screen *S* confines each eye to its own field of regard. The stages of conversion are: *a* — *b'* — *a* — *a'*: *L* sees the right eye's, and *R* the left eye's picture of the object. The objects viewed with this instrument must be small, and their range of distance is exceedingly limited.

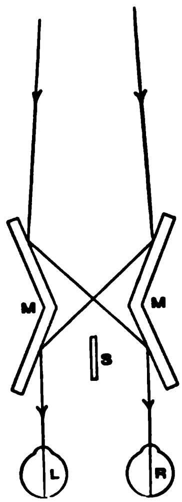


FIG. 80. — Ewald's mirror pseudoscope. Majer, Mk. 25.

(*c*) G. M. Stratton's pseudoscope is shown diagrammatically in Fig. 81 (see Psychol. Review, v., 1898, 632). *M* and *N* are mirrors, which can be turned about their vertical axes; *M* can also be moved to or from *N* in the horizontal line. *L* views the object directly; *R* views it after a double reflexion. The mirrors must be of good quality, or the loss of light in *R*'s image will affect the result. The conversion is of the first type, though the manner in which it is induced differentiates this instrument both from Ewald's pseudoscope and from the stereoscopes with interchanged diagrams. It is as if the right eye, with its normal image, were placed bodily to the left of the left eye: *cf.* the Helmholtz telestereoscope. Wheatstone makes a limited application of the principle, as follows. "Having taken a photograph of the object, which should be one the converse of which has a meaning, take two others at the same angular distance (say 18°), one on the right side, the other on the left side of the original. Of the three pictures thus taken, if the middle one be presented to the right eye, and the left picture to the left eye, a normal relief will be

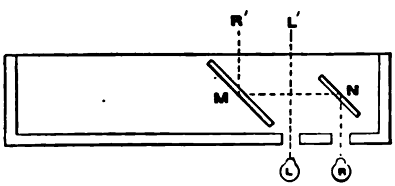


FIG. 81. — Stratton's mirror pseudoscope. A working model can be made for $3.

seen; but if the right picture be presented to the left eye, the other remaining unchanged, a converse relief will be seen." Similar results are found, if the left eye sees the middle picture, and the right the right and left pictures successively. "It must be observed, that the normal and converse reliefs, when the same picture remains presented to the same eye, belong to two different positions of the object" (11).

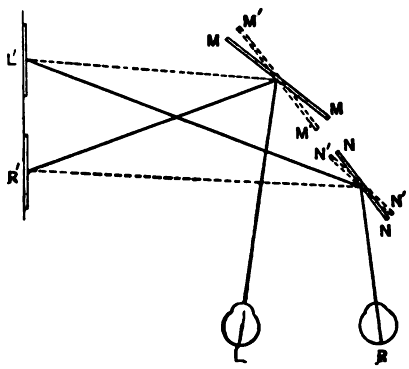


FIG. 82.

(*d*) J. Jastrow's adaptation of the 'perspectoscope' (Psychol. Review, vii., 1900, 53). *MM* and *NN* are two mirrors, whose positions can be changed to *M'M'* and *N'N'*. *R'* and *L'* are the centres of the right and left pictures of an ordinary Brewster stereogram. If the mirrors are at *MM* and *NN*, the instrument is a pseudoscope: *R* sees *L'* and *L* sees *R'*. The stages of conversion are, again, $a - b' - b$. If the mirrors are at *M'M'* and *N'N'*, the instrument is a stereoscope; *R* sees *R'* and *L* sees *L'*.

(2) *Lenticular Pseudoscopes.*—(*a*) Wheatstone's pseudoscope. "Place between the object and each eye a lens of small focal distance, and adjust the distances of the object and the lenses so that distinct inverted images of the object shall be seen by each eye; on directing the eyes to the place of the object the two images will unite, and the converse relief be perceived. . . . The field of view is very small, on account of the distance at which it is necessary to place the lenses from the eyes. . . . The inverted images of the lenses may be thrown upon a plate of ground glass as in the case of the ordinary camera obscura, and may be then caused to unite by the means employed in any form of the refracting stereoscope" (16). (*b*) Wood's pseudoscope puts this idea of Wheatstone's into compendious form. It consists of a Brewster stereoscope, from which the slide carrier and bar have been removed, and which is fitted with a pair of black-

ened tubes screwed to the posterior surface of the hood. Within these tubes slide two others, closed at their farther ends by double convex lenses. An extra handle enables the observer to adjust the length of tube to suit his eyes. Conversion is of the third type (inversion). This somewhat impairs the value of the instrument for general purposes: on the other hand, the field is large and clear. — Jastrow, Psychol. Rev., vii., 48; R. W. Wood, Science, Novr. 3, 1899.

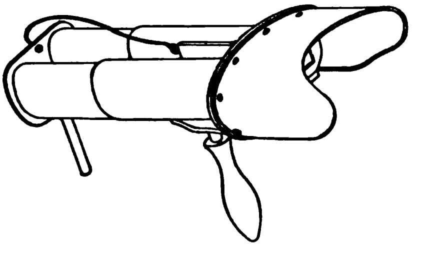


FIG. 83. — Wood's lenticular pseudoscope. Chicago Lab. Supply Co., $8.

PRELIMINARIES AND QUESTION (2). — We may again quote Wheatstone. "When the pseudoscope is so adjusted as to see a near object while the optic axes are parallel, to view a more distant object with the same adjustment the axes must converge, and the more so as the object is more distant; all nearer objects than that seen when the axes are parallel, will appear double, because the optic axes can never be simultaneously directed to them. If this instrument be so adjusted that very distant objects are seen single when the eyes are parallel, *all* nearer objects will appear double, because the optic axes can never converge to make their binocular images coincide. If the attention is required to be devoted to an object at a particular distance, the best mode of viewing it with the pseudoscope is to adjust the instrument so that the object shall appear at the proper distance and of its natural size. In this case the more distant objects will appear nearer and smaller, and the nearer objects will appear more distant and larger" (12). The first part of the quotation accounts in some measure for the difficulty experienced by beginners in using the instrument; the latter part explains our fixation of the cross upon the screen.

EXPERIMENT (1). — The nearer ball or rod seems to be the more remote, the left-hand object the right-hand, and *vice versa*. The illusion persists when the number of objects is increased.

EXPERIMENT (2). — In every case there is conversion of the hoop-curvature.

EXPERIMENT (3). — It is probable that the cone will appear, for a second or two, in normal relief. Then the apex gradually begins to retreat; the whole cone 'telescopes,' and a hollow cone is perceived. In the second part of the experiment the same process is repeated, in the opposite direction. The conversion requires less and less time as practice is continued.

EXPERIMENT (4). — *O* must report, as accurately as possible, the appearance of the pseudoscopic field. He should note the fluctuations of relief that appear in one and the same object, and should attempt to grade the objects in order of difficulty of conversion. At first, as Sanford says, "the pseudoscopic effect seems quite capricious." Presently, however, as the observer comes to know what to look for, the objects sort themselves out: those are easily converted whose 'converse has a meaning,' *i.e.*, whose conversion is not opposed by central factors; and those change with difficulty or not at all whose converse is meaningless, *i.e.*, whose conversion is opposed by central factors, by the 'apperception' of the binocular image. If the cortex is set obstinately for 'cup' it is useless for the eyes to say 'sphere.' To analyse the central factors by introspection is exceedingly difficult. The visual images come to us, so to speak, with the recognition-mark upon them; and the only thing that militates against conversion, so far as introspection is concerned, may be some muscular attitude, or organic complex, which constitutes the 'feel' of familiarity. Logically, therefore, the classification of objects as just proposed is fairly easy; psychologically, we have no criterion other than the intensity of the recognition-mark. — Wheatstone, 13; Külpe, Outlines, 171; Titchener, Outline, 275; Bentley, Amer. Journ. of Psych., xi., 1 ff.

We assume, in the above discussion, that monocular criteria are ruled out. When we turn the pseudoscope upon a group of objects (whether these are arranged before the screen or regarded as they lie in the room or landscape) such criteria inevitably come into play.

EXPERIMENT (5). — The more remote ball appears to *O* to be swinging not in a straight line but in a circle or ellipse. As it

disappears behind the near ball, it looks farther off; as it reappears, it comes to the front again.

The pseudoscopic effect is destroyed near the table, but persists above. If it persists completely, the pencils seem to be crossed; if it persists only partially, they incline towards each other.

The experiment shows clearly the effect of the fourth monocular criterion: interposition.

Experiment (6). — As *O* looks along the line *mn*, the large square readily comes up to the front of the two middle-sized squares. The small square, on the other hand, lags behind. Even if it comes to the front of the two middle-sized squares, it is still farther off in the pseudoscopic field than the largest square. The experiment shows the effect of the fifth criterion: magnitude of retinal image.

Experiment (7). — As *O* looks along the line *mn*, the two farther rings (white and grey) come up before the two front rings (white). It will be noticed, however, that the grey ring lags behind its white companion; so that, under favourable conditions, the far white ring, the grey ring, and one of the near white rings seem to lie in the same straight line, the far white ring nearest the observer.

The experiment succeeds best in a dull twilight. In any case, the light must be distributed with perfect evenness over the screen and wires.

If the laboratory has no suitable grey-covered wire, a black wire may be taken and lightly chalked over. It should be noticed that, if the four rings are observed in monocular vision, without the pseudoscope, the grey ring looks farther off than the corresponding white ring (second criterion; indistinctness of outline). This illusion is strong enough to persist under pseudoscopic conditions.

Experiment (8). — The one cone telescopes readily; the other with difficulty, or not at all. If *O* knows the side from which the illumination came, he will probably say positively that the originally hollow cone looks hollow, while the originally projecting cone merely flattens or undergoes a very gradual inversion. If he does not know, he will be able to invert either cone,

according to the side from which he supposes the light to be coming. The experiment shows the effect of the third criterion: distribution of light and shade.

Experiment (9). — The hollow mask is very easily converted into a projecting face. On the other hand, the mask-face is only with very great difficulty convertible into a hollow. Its appearance is changed: the nose seems to be driven into the face, and the chin and forehead protrude abnormally; but, for a long time, it persists as a face. Steady fixation will, in most cases, secure the required conversion, — especially if *O* has handled the mask beforehand, and is thus familiar with the look of the painted interior.

In this case, the apperceptive or central factors are, at first, strong enough entirely to outweigh the perceptive or peripheral. In the case of the human face, the central factors are still stronger. James says that the features of the living face obstinately refuse to be converted by the pseudoscope (Princ. of Psychol., ii., 258). Wheatstone, however, effected the conversion after "a fixed stare of more than half an hour" (Edinburgh Review, 1858, 460). The author once succeeded in obtaining such a conversion, and in maintaining it for a few seconds, at a time when he had had a very unusual course of practice with busts and casts, some of which were tinted: but the success has remained unique. — For a study of central *vs.* peripheral processes, see Pillsbury, Amer. Journ. of Psychol., viii., 315 ff.

Question (3) No. "The refraction of the rays of light at the incident and emergent surfaces of the prisms enables the reflexion of an object to be seen when the object is even behind the prolongation of the reflecting surface, . . . and thus the binocular image may be seen in the same place as the object itself, whereas the images cannot be made by plane mirrors thus to coincide" (Wheatstone). Let the student demonstrate this statement by a diagram.

(4) "The reason is this: that [in the pseudoscope] the projections to each eye are separately reflected, still remaining presented to the same eye, whereas, by the reflexion of the object itself [in a mirror], not only are the projections reflected, but they are also transposed from one eye to the other; and

these circumstances occurring simultaneously reproduce the normal relief" (Wheatstone). In terms of Fig. 25 of the text, a mirror will change *a* to *b'*. The same reason may be given for the fact that inversion of an actual object does not convert its relief (*a* becomes *c'*). The student should, again, be required to draw explanatory diagrams.

(5) "The conversion of distance" in the pseudoscope, like the perception of distance in the stereoscope, "takes place only within those limits in which the optic axes sensibly converge, or the pictures projected on the retinæ are sensibly dissimilar. Beyond this range there is no mutual transposition of the apparent distances of objects with the pseudoscope; a distant view therefore appears unchanged" (Wheatstone).

EXPERIMENT XXIX

§ 49. **Visual Space Perception: the Geometrical Optical Illusions.** — The main current of work in a science is interrupted, from time to time, by some eddy of special interest. A few years ago the 'kinæsthetic' sensations were attracting what seemed to be more than their due share of psychological attention; and more recently the 'geometrical optical illusions' have loomed very large in the pages of the psychological journals. The right way to approach a subject of this kind is to take the literature as a whole: to trace the conditions which have led several observers, independently, to a study of the same or similar phenomena, and which have prepared still other workers in the field to offer expert criticism, at short notice, of the results and theories first published; and, in the light of these conditions, to read synoptically all that has been written, not losing oneself in details, but keeping watch throughout for the broader psychological principles that underlie the detailed discussions. There must, in the present case, have been something in the psychological atmosphere that was favourable to the growth of an illusion-literature; and the profit to be drawn from this literature is, most assuredly, not the mere collection of possibilities of explaining a particular figure, — though the understanding of these possibilities is no small matter, and the experimental

methods devised for the study of particular figures are no small gain to the science, — but rather the grasp of principles: the clarifying of one's idea of spatial contrast, for instance, or the weighing of arguments for and against the 'perception' and the 'judgment' theories of optical illusions at large, or the estimation of a 'genetic' as against a 'physiological' or 'nativistic' theory of space perception.

Unfortunately, study and appreciation of this sort demand more time and more knowledge than are available in a first laboratory course. The teacher is therefore met by the old pedagogical difficulty. Shall he try to work up the material into a coherent system, at the risk of being one-sided? Or shall he take the student over the whole ground, at the risk of being scrappy? The author, after experience of both alternatives, has decided in favour of the former. In following Wundt's exposition, the student is, perhaps, — one might almost say 'is probably' — led to underestimate the complexity of the problems before him. But, at any rate, he learns a method; he realises that the way to solve a problem is to grapple with it steadfastly, consistently, systematically. On the other hand, if he is set down before separate illusion-figures, and required to tabulate all the principles of explanation that different writers have employed, he comes to think (what is emphatically not true) that these 'principles' are very much a matter of guesswork, and that one way of explaining a psychological phenomenon is as good as another. He has not the perspective that would enable him to refer the various explanations to their proper psychological places; he has not traced conditions.

There is a further point. The one-sidedness can be corrected more easily and effectually than the scrappiness. The author has been accustomed, at the conclusion of the Experiment, to run over in class the proposed explanations of the Müller-Lyer (arrowhead and feather) illusion. The variety of factors to which these explanations appeal comes with something of a shock to the student. But the Instructor can lessen the shock, by giving the novel principles their historical and systematic setting; while the student still has the Wundtian canons firmly in mind, can compare the range of the new principles with their

range, can always come back to them when he seems to be losing his bearings in the multitude of details, and may very well be incited by the clash of ideas to investigation on his own account. Scrappiness, on the contrary, is apt to mean a self-satisfied dilettantism. You can meet a prejudice by giving it the lie direct; but how are you to convince a shallow mind that other minds are deeper than itself?

The following are the most important literary references.

(1) *Systematic Discussions.* — W. Wundt, Die geometrisch-optischen Täuschungen. Leipzig, Teubner, 1898.

Th. Lipps, Raumaesthetik und geometrisch-optische Täuschungen. Leipzig, Barth, 1897.

A. Thiéry, Ueber geometrisch-optische Täuschungen. In Wundt's Philosophische Studien, xi., 1895, 307, 603; xii., 1896, 67. Emphasises perspective as a principle of explanation.

E. C. Sanford, A Course in Experimental Psychology, 1898, 212 ff. Diagrams, with "brief commentary . . . intended merely as a suggestion of the views held with regard to them, not as an exposition or criticism of those views."

J. I. Hoppe, Psychologisch-physiologische Optik. Leipzig, Wigand, 1881.

To these may be added: Helmholtz, Phys. Optik, 2d edn., 1896, 705; Wundt, Phys. Psych., 4th edn., 1893, ii., 137; James, Princ. of Psych., 1890, ii., 231, 248, 264; Bowditch, in Howell's American Textbook of Physiology, 1896, 789–806.

(2) *Illusions of Reversible Perspective.* — H. Beaunis, Nouveaux éléments de physiologie humaine. Paris, 1888, ii., 569.

W. Filehne, Zeits. f. Psych., xvii., 1898, 19.

H. von Helmholtz, Phys. Optik, 771.

E. Hering, in Hermann's Handbuch der Physiologie, iii., 1, 1879, 579.

J. I. Hoppe, Psych.-phys. Optik, 64, 203, 251, 274.

W. James, Princ. of Psych., ii., 254–256, 265.

J. Jastrow, Pop. Sci. Monthly, xxxiv., 1889, 150; liv., 1898–9, 306.

N. Lange, Philos. Studien, iv., 1888, 406.

T. Lipps, Raumaesthetik, 73.

J. Loeb, Pflüger's Archiv, xl., 1887, 274, 281.

E. Mach, Beiträge zur Analyse der Empfindungen, 1886, 87, 94, 96 f.; Eng. trans., 1897, 91, 99, 101.

L. A. Necker, Poggendorff's Annalen, xxvii., 1833, 497.

J. Oppel, Poggendorff's Annalen, xcix., 1856, 466.

E. C. Sanford, Course, 215, 255.

H. Schröder, Poggendorff's Annalen, cv., 1858, 298.

A. Thiéry, Philos. Studien, xi., 1895, 317.

C. Wheatstone, Phil. Trans., 1838, 381.

S. Witasek, Zeits. f. Psych., xix., 1899, 81–174.

W. Wundt, Phys. Psych., ii., 200; Täuschungen, 58 ff.; Philos. Studien. xiv., 1898, 27 ff.

(3) *Variable Illusions of Extent.* — H. Aubert, Physiol. d. Netzhaut, 1865. 264.

F. Auerbach, Zeits. f. Psych., vii., 1894, 152.

A. Binet, Revue philosophique, xl., 1895, 11; Année psychologique, i., 1894, 328.

J. J. van Biervliet, Revue philosophique, xli., 1896, 169.

F. Brentano, Zeits. f. Psych., iii., 1892, 349; v., 1893, 61; vi., 1893–4, 1

C. Brunot, Revue scientifique, lii., 1893, 210.

J. Delbœuf, Bull. de l'Acad. roy. de Belgique, 3 série, xxiv., 1892, 12; Revue scientifique, li., 1893, 237.

W. Einthoven, Pflüger's Archiv, lxxi., 1898, 1.

H. von Helmholtz, Phys. Optik, 705.

E. Hering, Beiträge zur Physiologie, i., 1861, 66; Hermann's Hdbch., iii., 1, 554.

G. Heymans, Zeits. f. Psych., ix., 1895–6, 221.

J. Jastrow, Amer. Journ. of Psych., iv., 1891–2, 396.

H. W. Knox and R. Watanabe, Amer. Journ. of Psychol., vi., 1893–5, 413, 509.

O. Külpe, Outlines of Psych., 1895, 366.

A. Kundt, Poggendorff's Annalen, cxx., 1863, 128.

W. Láska, Du Bois-Reymond's Archiv, 1890, 326.

T. Lipps, Zeits. f. Psych., iii., 1892, 501; Raumaesthetik, 70, 141 (146, 161; 150; 363; 237, 241, 251, 254, 364; 128, 135, 137; 72).

J. Loeb, Pflüger's Archiv, lx., 1895, 509.

H. Messer, Poggendorff's Annalen, clvii., 1876, 172.

F. C. Müller-Lyer, Du Bois-Reymond's Archiv, 1889, S. B., 263; Zeits. f. Psych., ix., 1895, 1; x., 1896, 421.

J. Oppel, Jahresber. d. physikal. Ver. zu Frankfurt a. M., 1856–7, 51; 1860–1, 35.

E. C. Sanford, Course, 229, 233.

A. Thiéry, Philos. Studien, xii., 1896, 67.

W. Wundt, Phys. Psych., ii., 142; Täuschungen, 82.

(4) *Constant Illusions of Extent.* — A. Chodin, Arch. f. Ophthalmologie, xxiii., 1, 1877, 99.

J. Delbœuf, Bull. de l'Acad. roy. de Belgique, 2 série, xix., 2, 1865, 9.

R. Fischer, Arch. f. Ophthalmologie, xxxvii., 1, 1891, 97; xxxvii., 3. 1891, 55.

H. von Helmholtz, Phys. Optik, 684, 702.

E. Hering, Beiträge zur Physiol., v., 1864, 355; Hermann's Hdbch., iii., 1, 1879, 553.

W. Holtz, Wiedemann's Annalen, x., 1880, 158.

J. I. Hoppe, Psych.-phys. Optik, 158, 351.
A. Kundt, Poggendorff's Annalen, cxx., 1863, 118.
T. Lipps, Helmholtz Festgruss, 1891, 221; Raumaesthetik, 266, 293.
H. Münsterberg, Beiträge z. experiment. Psych., 2, 1889, 125.
J. Oppel, Jahresber. d. physikal. Ver. zu Frankfurt a. M., 1854–5, 38.
E. C. Sanford, Course, 235.
A. Thiéry, Philos. Studien, xii., 1896, 93.
W. Wundt, Beiträge z. Theorie d. Sinneswahrnehmung, 1862, 158; Phys. Psych., ii., 137; Täuschungen, 105.
(5) *Variable Illusions of Direction.* — H. Aubert, Physiol. Optik, 1876, 630.
S. Bidwell, Curiosities of Light and Sight, 1899, 141.
E. Burmester, Zeits. f. Psych., xii., 1896, 355.
J. Delbœuf, Bull. de l'Acad. roy. de Belgique, 2 série, xix., 2, 1865, 195; Revue scientifique, li., 1893, 237.
F. B. Dresslar, Amer. Journ. of Psych., vi., 1893–5, 275.
W. Einthoven, Pflüger's Archiv, lxxi., 1898, 4.
W. Filehne, Zeits. f. Psych., xvii., 1898, 15.
C. L. Franklin, Amer. Journ. of Psych., i., 1887–8, 99.
A. A. Guye, Revue scientifique, li., 1893, 594.
H. von Helmholtz, Phys. Optik, 705, 707, 708, 712.
E. Hering, Hermann's Hdbch. d. Physiol., iii., 1, 1879, 372; Beiträge z. Physiol., i., 1861, 73, 79.
G. Heymans, Zeits. f. Psych., xiv., 1897, 101.
W. Holtz, Göttinger Nachrichten, 1893, 159.
J. I. Hoppe, Psych.-phys. Optik, 74.
J. Jastrow, Amer. Journ. of Psych., iv., 1891–2, 381; Pop. Sci. Monthly, liv., 1898–9, 304.
O. Külpe, Outlines of Psych., 1895, 367.
A. Kundt, Poggendorff's Annalen, cxx., 1863, 121, 148.
T. Lipps, Helmholtz Festgruss, 1891, 267; Raumaesthetik, 257 (223, 265, 307; 307, 319; 263, 274, 371); Zeits. f. Psych., xviii., 1898, 440.
E. Mach, Beiträge z. Analyse d. Empfindungen, 1886, 98; Eng. trans., 1897, 41.
F. C. Müller-Lyer, Du Bois-Reymond's Archiv, 1889, S. B., 263.
H. Münsterberg, Zeits. f. Psych., xv., 1897–8, 184.
J. Oppel, Jahresber. d. physikal. Ver. zu Frankfurt a. M., 1854–5, 41.
A. H. Pierce, Psychol. Rev., v., 1898, 233; vii., 1900, 356.
E. C. Sanford, Course, 218, 219, 224.
A. Thiéry, Philos. Studien, xi., 1895, 312, 360, 607; xii., 1896, 74.
K. Ueberhorst, Zeits. f. Psych., xiii., 1896–7, 59.
A. W. Volkmann, Physiol. Untersuchungen im Gebiete d. Optik, i., 1863, 163.
W. Wundt, Phys. Psych., ii., 144; Täuschungen, 113.
W. von Zehender, Zeits. f. Psych., xx., 1899, 79, 83, 85, 103.
F. Zöllner, Poggendorff's Annalen, cx., 1860, 500; cxiv., 1861, 587.

(6) *Constant Illusions of Direction.* — F. C. Donders, Arch. f. Ophthalmologie, xxi., 3, 1875, 100.
H. von Helmholtz, Phys. Optik, 694, 862.
E. Hering, Hermann's Hdbch. d. Physiol., iii., 1, 1879, 355, 371.
F. Küster, Arch. f. Ophthalmologie, xxii., 1, 1876, 149.
F. von Recklinghausen, Arch. f. Ophthalmologie, v., 2, 1859, 127.
E. C. Sanford, Course, 191, 268.
A. W. Volkmann, Physiol. Unt. im Gebiete d. Optik, i., 1863, 220.
W. Wundt, Phys. Psych., ii., 129, 141; Täuschungen, 130.

(7) *Illusions of Association.* — H. Aubert, Physiol. Optik, 1876, 629.
J. M. Baldwin, Psychol. Rev., ii., 1895, 244.
T. Lipps, Helmholtz Festgruss, 300; Zeits. f. Psych., xii., 1896, 52; Raumaesthetik, 100; 104 f.
A. Höfler, Zeits. f. Psych., x., 1896, 99.
J. Loeb, Pflüger's Arch., lx., 1895, 509.
F. C. Müller-Lyer, Du Bois-Reymond's Arch., 1889, S. B., 263; Zeits. f. Psych., ix., 1895, 3; x., 1896, 421.
E. C. Sanford, Course, 238, 246, 253.
A. Thiéry, Philos. Studien, xii., 1896, 83.
W. Wundt, Phys. Psych., ii., 146, 150; Täuschungen, 137.
W. von Zehender, Zeits. f. Psych., xx., 1899, 106 f.

(8) *Illusions with Complication of Conditions.* — J. Delbœuf, Revue scientifique, li., 1893, 237; Bull. de l'Acad. roy. de Belgique, 2 série, xx., no. 6, 1865, 70.
F. B. Dresslar, Amer. Jour. of Psych., vi., 1893–5, 275.
H. von Helmholtz, Phys. Optik, 707.
E. Hering, Hermann's Hdbch. d. Physiol., iii., 1, 1879, 372.
G. Heymans, Zeits. f. Psych., xiv., 1897, 117.
T. Lipps, Helmholtz Festgruss, 1891, 233, 290; Raumaesthetik, 321 (389, 398; 108, plate at end; 72, 291; 313, 317); Zeits. f. Psych., xv., 1897–8, 137; xviii., 1898, 433, 435.
J. Loeb, Pflüger's Archiv, lx., 1895, 512.
W. Láska, Du Bois-Reymond's Archiv, 1890, 326.
F. C. Müller-Lyer, Du Bois-Reymond's Archiv, 1889, S. B., 263; Zeits. f. Psych., x., 1896, 421.
J. Oppel, Jahresber. d. physikal. Ver. zu Frankfurt a. M., 1856–7, 48.
E. C. Sanford, Course, 227, 243, 246, 251.
A. Thiéry, Philos. Studien, xi., 1895, 357; xii., 1896, 94, 108.
W. Wundt, Phys. Psych., ii., 148, 151; Täuschungen, 145.

(9) *General Theoretical Discussions.* — E. B. Delabarre, Amer. Journ. of Psych., ix., 1898, 573.
T. Lipps, Raumaesthetik, 1–69 (esp. 61 ff.); Zeits. f. Psych., xii., 1896, 39.
A. Thiéry, Philos. Studien, xi., 1895, 307, 603; xii., 1896, 67.

S. Witasek, Zeits. f. Psych., xix., 1899, 81.
W. Wundt, Täuschungen, 157; Philos. Studien, xiv., 1898, 1.
W. von Zehender, Zeits. f. Psych., xviii., 1898, 91–98.

We come now to the Experiment, with Wundt's figures and interpretations. The diagrams which show these illusions have become more or less common property; but the author has attempted to refer them to their first inventors or observers. For more careful work than the Experiment demands, the figures should be drawn on a larger scale and on separate sheets of paper, so that they stand practically alone in the visual field. Those that show perspective may be constructed of narrow strips of white paper pasted on black cardboard backgrounds. Whether or not the student make these larger diagrams for himself, the Instructor should have a few prepared as large wall-diagrams: the author recommends for this purpose Schröder's stair-figure, Necker's cube, Helmholtz' cross-lined squares, the Müller-Lyer figure, the Oppel-Delbœuf-Kundt cross, Hering's parallels, Wundt's parallels, Zöllner's figure, Helmholtz' chess-board (von Recklinghausen's illusion), Poggendorff's figure and Müller-Lyer's broken circle. Some authors advise the demonstration of illusions by means of wire models; and it is true that the apparent lengthening or shortening of a piece of wire is more striking than the lengthening or shortening of a pen-stroke. Half-a-dozen pairs of wires, showing various forms of the Müller-Lyer illusion, can be obtained from any tinsmith, or made in the Laboratory; they are very effective for class purposes. The author has also used for some years a large movable model of the Müller-Lyer figure: two strips of black card (the two constants) are pasted upon a sheet of heavy millboard, and a number of loose back strips, of varying length, provided. The loose strips are put together, V-wise, by a pin, which is then thrust through the extremity of the fixed line. It is thus possible to vary the length and the angle of the oblique attachments, and to modify or convert the illusion before the eyes of the observer. The same principle is employed in Münsterberg's Pseudoptics.

It should be noted that Wundt is followed, in this Experiment, only to his *proximate* interpretations. How, for instance, the

increase or decrease of expenditure of muscular energy comes to have an influence, on his theory, upon our perceptions of space, is a point that is not here explained. The explanation must come in a systematic lecture course. The points emphasised here are the factual influence of fixation and eye-movement, especially in the illusions of reversible perspective, and the verification of this influence by introspection throughout the five principal illusion-series. At the conclusion of the Experiment, and before the Questions are attempted, the contents of Wundt's final section (§ 10, 168 ff.) — except where the answers to the Questions are anticipated by it — should be given to the student.

EXPERIMENT (1). — (i.) Fixation of *a* in *A* and *B* brings *a* out, towards the observer. The figures are thus seen in perspective as crosses: the line *cd* is constant in the plane of the paper, the limb *ab* stretches across it, into the space behind the paper. As the eye moves to *b*, the point *b* comes out to the observer; the perspective is reversed. Note the tendency (not mentioned in the text) to see the angles of intersection all alike as right angles. — In *C* and *D* there is no line of constant orientation; it is only the point of intersection that remains in the plane of the paper. Hence fixation of *a* brings out both *a* and *d* towards the observer; fixation of *b* brings out both *b* and *c*. — The two perspectives may be obtained with *E* and *F*. There is, however, a tendency to regard the lower part of a linear figure as the part that is nearer to the observer. Hence the illusion with fixation of *b* is more difficult to obtain than that with fixation of *a*. Notice the influence of this tendency in *C* and *D*. — *G* and *H* repeat the illusions of *A* and *B*. We see telegraph-poles in place of the right-angled crosses. — The student's attention should be called to the extreme difficulty with which any considerable perspective effect is obtainable from a simple vertical or simple horizontal line.

(ii.) As a general rule, *A* is seen with the edge *be* convex. Fixation of any point on *bc*, or movement along it, maintains this perspective. Fixation of any point upon *ad* or *cf* brings these lines forward. Movement from *a* or *c* to *b*, and movement from *f* or *d* to *c*, render *bc* concave. The opposite movements restore its convexity.

Note the tendency to see the figures *abed*, *bcfe* as right-angled parallelograms.

Loeb's Experiments. — At a moment when the figure appears convex, move it rapidly away from the observing eye: the perspective changes. Now move it quickly in again: the convexity is restored. The reason is, that movement away, when some point on *be* is under fixation, necessitates an eye-movement in the direction *ab* or *cb*; whereas movement towards the observer implies eye movement in the direction *ba* or *bc*. — Bring up a pencil-point between the figure and the observing eye. Move it towards the figure: *be* is concave. Move it towards the eye: *be* is convex. The reason is, again, that movement of the point towards the figure means an eye-movement in the direction *ab* or *cb*, while movement towards the observer means eye-movement in the direction *ba* or *bc*. If the movement of figure or pencil is so slow that the eye can maintain its original fixation, there is no shift of perspective.

Figure *B* is ordinarily seen as a tetrahedron, with the edge *db* convex. Fixation of a point on this edge, or movement of the eye in the directions *ba*, *bc*, maintains this perspective; fixation of a point upon *ac*, *ad* or *cd*, and movement of the eye in the directions *ab*, *cb*, reverse the perspective. The secondary modifications are transparency and shift of orientation. The line *ac* is seen behind *bd*, or *vice versa*; and the vertical axis of the tetrahedron inclines in the one case towards, in the other case away from the observing eye.

Loeb's experiments may be repeated with this figure.

The figure is capable of four other interpretations. It may appear, not as a tetrahedron at all, but as a figure composed of the junction of two plane triangles. Fixate the point *b*: the triangle *abc* is turned towards the observer. Fixate some point upon *ac* (*e.g.*, the point of intersection of *ac* and *bd*): the triangle *abc* is bent away from the observer. The fixation must be steady and continuous; and the least eye-movement means that the figure slips back into its tetrahedral form. Finally, the figure may appear as a pentahedron, a solid or hollow four-sided pyramid, whose apex is the point of intersection of *ac* and *bd*. The illusions are more striking if the figure be turned through 45° (*cf.* *A* and *B* of Fig. 29, Pt. i.) and the sides *ad*, *cd* somewhat shortened. The conditions of the two perspective interpretations can easily be worked out by the student.

(iii.) The figure appears ordinarily as a flight of steps. If *a*

be fixated, or the eye move from *a* to *b*, this perception persists. If *b* be fixated, or the eye move from *b* to *a*, the figure appears as an overhanging portion of a wall.

If *a* is fixated, and this fixation maintained while the figure is turned through 180°, the converse relief must, of course, appear. This is the explanation of Schröder's statement, that the inversion of perspective occurs most easily with inversion of the diagram. Intrinsically, there is as strong a tendency to see the flight of steps (to fixate *b* or *c*) when the figure is inverted as there is to see it (to fixate *a*) when the figure is in its normal position. The tendency to fixate the lower end of an oblique line drawn in perspective, and to follow the lines of fixation from below upwards rather than from above downwards, has been noted above under (i.).

(iv.) The tendency is to see the edge *ab* as nearer the observer. Fixation of any point upon *ab*, and eye-movement in the directions *bf*, *bc*, maintain this perspective; fixation of a point on *gh*, and eye-movement in the directions *gf*, *gc*, reverse it. A right-hand turn of the figure through 90°, making *cd* horizontal, exaggerates the tendency to see the edges *bc*, *cd* as convex. Contrariwise, a left-hand turn, making *ef* horizontal, exaggerates the tendency to see the edges *ef*, *eh* as convex. These turns of the figure, therefore, facilitate the reversal of perspective. The reason is that the fixation-lines are brought farther from the horizontal, and that the general suggestion of perspective is thereby enhanced.

Note that the figure in perspective is not that of a true cube; the farther side appears too large. It follows that the angles of the figure are not all seen as right angles. Note also that the conversion of perspective is always accompanied by an apparent turn of the whole figure about a horizontal axis.

Two other illusions are possible. Fixate steadily and continuously an imaginary point lying midway between *ab* and *gh*. The edges *ab* and *gh* both appear convex; there is no solid cube, but two roofs, or two book-covers, crossing each other in a somewhat baffling way. Now fixate an imaginary point midway between *ab* and *ef*, or *gh* and *cd*. The edges *ab* and *gh* are both concave; the roofs or covers are open towards the observer. Neither illusion is easy to obtain; the former is the less difficult of the two.

(v.) Fixation of the centre of the surface *abcd* of *A* gives the illusion of a solid prism. Fixation of points on *ce*, *bf* or *dg* brings these edges out towards the observer: this accords with our previous rules. Movement of the eye from any one of these lines along an oblique line does not alter the perspective until the line *hi* or *kl* has been passed; beyond this point, the perspective is reversed, *i.e.*, the apparent irregularity comes into play. Movement of the eye along *hi* or *kl* produces a quick and confusing alternation of reliefs. Movement from these lines along an oblique line, if directed towards *ce*, makes the prism concave; if directed towards *bf* or *dg*, makes it convex. These results are regular.

The student can work out for himself the corresponding illusions of *B*. We may have, from left to right, hollow-solid, solid-hollow, or solid-solid. The conditions must be carefully noted.

EXPERIMENT (2).—(vi.) It is natural to estimate the lines and distances of *A*, *B* and *C* by eye-movement; in *D*, however, the middle division of *a* arrests the eye, and the tendency is to take in the whole line at once, by a single fixation. The obvious thing, in the way of illusion, is the different apparent length of the objectively equal distances. In *A*, *b* is the longer; in *B*, *b*; in *C*, *a* and *b* are longer than the open space; in *D*, however, *a* is shorter than *b*.

There is a weak illusion of perspective. In *A*, *a* is nearer than *b*; in *B*, *a* is nearer; in *C*, there is no perspective, only an empty space between *a* and *b*; in *D*, *a* is nearer. *C* shows that the illusion of extent is primary, since we have in it an illusion of extent with no illusion of perspective at all: if the latter were the primary illusion, we could never have an illusion of extent without the presence of the conditioning perspective factor.

The illusion of extent is fairly constant, whatever the position of the figures; the illusion of perspective is strongest when the distances are horizontal. The illusion of extent is more plainly seen with eye-movement, the illusion of perspective with steady fixation.

E is merely a variant of *C*: *a* looks higher, and *b* looks wider than the objectively equal square *c*. Both *a* and *b* are seen in the same plane, while *c* is somewhat nearer the observer.

"Distances, the traversing of which requires a movement of regard that is interrupted by fixation-points or prescribed by fixation-lines, appear longer than distances that can be traversed without fixation-points or in complete freedom, without prescription of path" (Wundt).

(vii.) The natural way of observing *A* is to take the horizontal line as line of fixation, and to estimate the length of the lines *a*, *b* by dropping imaginary perpendiculars from their points to the horizontal. In *A*, *b* is accordingly longer than *a*. In *B* and *C*, where the eye must traverse the lines *a*, *b* in succession, and no simultaneous projection upon a horizontal is possible, the illusion disappears. It recurs in *D*, and (though to a less degree) in *E*.

(viii.) The *b*-vertical is in every case longer than the *a*-vertical. The illusion is most marked in *A*. All three figures show a perspective illusion. In *C*, the illusion is that the middle line of *a* is nearer, and the middle line of *b* farther, than the lateral parallels. In *A* and *B*, the whole figure *a* is nearer than *b*. Moreover, the oblique pieces in *A* give a reversible perspective. If the eye passes from the vertical to the oblique lines, the figures appear as a roof or ridge, convex to the observer; if the eye passes from the oblique lines to the vertical (as may easily happen in *b*), the latter is more remote from the observer.

Both illusions (extent and perspective) persist when the figures are turned. The illusion of extent persists whether the eye be moving or steadily fixating; the illusion of perspective is enhanced by steady fixation.

"A distance which, in virtue of its fixation-lines, offers a motive to the continuance of movement in the same direction is adjudged greater, and a distance which, in virtue of similar lines in the opposite direction, offers a motive to the inhibition of movement is adjudged less, than an objectively equal distance in the traversing of which such motives are not operative" (Wundt). The lengthening of the *b*-lines is thus analogous to the greater length of *b* in *A* and *B*, Fig. 34 of Pt. i.; the shortening of the *a*-lines is analogous to the shortening of *a* in *D* of the same Fig. Note that the word 'motive,' in the quotation, is used in a special and technical sense.

The closed semicircle in *D* is, in principle, the half of *A a*; the diameter is underestimated, and the whole surface of the figure correspondingly lessened. The squares of *E* are a variant of the *B* figures.

EXPERIMENT (3).—(ix.) *A* presents one monocular and two binocular illusions. In binocular vision, the vertical line appears longer than the horizontal (Oppel); and the upper vertical limb appears longer than the lower (Delbœuf). In monocular vision the outer horizontal limb appears longer than the inner (Kundt).

B, C, D and *E* show Oppel's illusion; *B* shows Delbœuf's; *E* shows Kundt's. The author has not succeeded in seeing Kundt's illusion in *B*; the illusion is weak at best, and in this case the influence of the square and circle counteracts it.

Wundt explains the illusions by reference to asymmetries of muscular action. The fact that *F* produces no illusion, while *B* does, bears out the hypothesis.

The angular illusions of *B*, which are very striking, and will probably be noticed by the student, do not fall under the present heading.

EXPERIMENT (4).—(x.) The line *ab* in *A* seems bent at *c*, in such a way that *a* and *b* are slightly lower than *c*. In *B* the bend is in the opposite direction: *a* and *b* are slightly higher than *c*. If *c* be steadily fixated, *A* appears as a four-rayed star: *d* and *e* point towards, *a* and *b* away from, the observer. *B* appears, under similar conditions, as a bent strip of metal or paper, with the edge *cd* near the observing eye, and the points *a*, *b* remote. At the same time (and this is a point that the student may miss) *d* seems to be somewhat nearer the eye than *c*, as if the upper portion of the convex edge were tilted towards the observer.

The general formula is that small angles (*dca*, *ecb* in *A*, and *dac*, *dbc* in *B*) are overestimated, and obtuse angles underestimated in comparison with them. This must be the primary illusion, since the perspective illusion is not reversible but constant. Moreover, we have the angle illusion, without any perspective illusion at all, in *B* of Fig. 37, Pt. i.

(xi.) The parallels in *A* seem to converge, those in *B* to diverge, to right and left: *A* is thus a complicated variant of the

A of Fig. 38 (Pt. i.), and *B* a similar variant of the *B*. The illusion is reversed, because the acute angles formed by the rays with the parallels lie on opposite sides of the lines in the two figures. It is strongest at the points *ac*, *bd*, because the angles here are more acute than they are towards the centre of the diagrams.

The illusion of perspective is constant; the points from which the rays diverge are always the points most remote from the observing eye. If the eye be allowed to play over the figures, the band *abdc* in *A* takes the appearance of a hollow half-hoop, narrower at the sides than in the distance, while in *B* it is a convex half-hoop, narrower in the middle than at the sides. With steady fixation, the general perspective effect of the figures is greatly increased, but the lines *ab*, *cd* appear parallel, as they are. This last point is important for Wundt's theory.

(xii.) There are two illusions of direction in *A*. The parallel vertical strips appear to converge and diverge alternately above and below; and the right and left halves of the oblique cross-pieces appear shifted vertically, each in the direction in which it is pointing. The first illusion is much more pronounced in *B* than in *A*; the second has no opportunity to show itself.

Both illusions are referrible to the overestimation of acute angles. If the acute angles which the oblique cross-pieces make with the verticals are subjectively increased, the shift of each half follows as a matter of course; and it is equally necessary that the verticals themselves shall diverge in the direction towards which the cross-pieces point, and converge in the direction from which they point.

Steady fixation of *A* diminishes (or even destroys) the illusion of direction, while it brings out an occasional plastic effect from one part or other of the figure. Steady fixation of *B* produces a very strong perspective effect. The lines that are crossed by horizontal pieces are turned with their upper ends towards the observer; the lines crossed by verticals have their lower ends towards the observer. The whole figure thus seems to consist of a number of white threads, stretched alternately in opposite directions of space. At the same time, the longer lines are seen

to be parallel, and their apparent convergence is referred to their different spatial direction. We have again, therefore, an instance of the compensatory effect of perspective which, according to Wundt, characterises this group of illusions.

(xiii.) At first glance, the two halves of the oblique line appear shifted, in the vertical direction, as in Fig. 40, *A* (Pt. i.). If the eye be moved slowly up and down *a*, the oblique pieces may seem to make a sharp turn inwards, above and below the points where they really strike the verticals. The two halves of the oblique line are then seen as continuous in direction; but each half has a little hook, where it touches the vertical: it follows the base of the triangle, instead of the side.

Steady fixation of a point on *a* brings out the perspective effect; the lower end of the oblique line is nearer the observer, and the two halves appear in the same straight line without any hook or inward turn. The vertical strip *a* is drawn open, and the whole figure shown in black on white, to avoid a possible complication by irradiation.

"Wherever mechanical movements are produced, of short duration — varying in the individual case — and under the same conditions, there will be a relatively greater expenditure of energy in the shorter than in the longer movements, since it requires more energy to set a definite movement going than to continue a movement that is already begun" (Wundt). It is, then, the relatively greater expenditure of muscular energy in crossing a small angle that determines our overestimation of it. Notice that this is merely a proximate principle of explanation: it still remains to be shown how muscular energy becomes translated into spatial terms.

Experiment (5). — (xiv.) The hyperbolas become straight lines, so that the figure looks like a chessboard. The squares are of equal size, about the centre of the figure; towards the periphery they may seem to grow larger, even though the lines of division are directly vertical and horizontal.

The perspective illusion in this case is that of a bowl, turned convexly to the observer; the bottom of the bowl is flat, and the whole surface is divided up into equal squares, — the apparently larger being interpreted as the more remote. If the eye

move over the figure, we see a concave bowl; the curved lines are now the determining factors in the perspective effect.

Wundt compares the illusions of this figure to the false torsions of the right-angled after-image projected on a plane surface (136). "With unmoved line of regard, we apprehend the directions of lines seen in indirect vision as they would necessarily appear in direct vision if the regard were transferred (with unchanged position of the retinal image) from the lateral parts of the visual field to its present point of fixation."

Experiment (6). — (xv.) The order, from longest to shortest, is: *c. a, b, d.* All four middle pieces are 20 mm. in length. In *a* and *b*, this 20 mm. is bounded by lines of 21 and 19 mm. respectively.

The lower part of the figure (*c, d*) shows a perspective illusion; *c* is more remote than *d*. There is no similar illusion in *a* and *b*.

Wundt regards these illusions as 'associative,' *i.e.*, as conditioned by purely psychological (not physiological) motives. In *a* and *b* we have an 'assimilative' association; lengths that are but slightly different are taken to be equal. Hence the middle line of *a* is overestimated, the middle portion of *b* underestimated. In *c* and *d* we have a 'contrastive' association: the middle portion of *c* is overestimated, and that of *d* underestimated, by 'contrast' with the outlying lines. The illusions of this class are but few in number; and we should, perhaps, rather be satisfied that so many forms of illusion have been brought under physiological rules than cavil at the exceptions. Nor is there anything intrinsically unscientific in the appeal to purely psychological conditions. Nevertheless, the author regards this section of Wundt's work as unsatisfactory, and hopes that it will presently be possible to subsume the refractory illusions to the laws of eye-movement and fixation.

(xvi.) *A* shows an assimilative 'interspace' illusion; the lines that are more widely separated appear the longer. *B* gives a contrastive interspace illusion.

Experiment (7). —(xvii.) In Fig. 45 (Pt. i.) the height of *A* and *B*, and the length of the horizontal boundary lines, are equal. But *B* looks higher than *A*, and the horizontal lines of

A look longer than those of *B*. We have as conditions a constant illusion of extent (the overestimation of vertical as compared with horizontal distances), and a variable illusion of extent (Müller-Lyer figure).

(xviii.) The illusion consists in the apparent vertical disjunction of the parts of the oblique line. We have as conditions: (1) the overestimation of acute angles (variable illusion of direction: *cf.* Pt. i., Fig. 41); (2) the overestimation of vertical as compared with horizontal distances (constant illusion of extent); and (3) the overestimation of filled space (here the vertical lines) as compared with empty space (the empty interior of the figure: a variable illusion of extent: *cf.* Pt. i., Fig. 34, *B*). All three conditions work in the same direction.

(xix.) The small arc seems to be concentric with the large arc, but to belong to a circle of greater radius. We have (1) an overestimation of the small arc, analogous to the overestimation of small angles (variable illusion of direction). As the curvature is not altered, the arc must belong to a circle of greater radius. (2) By an 'assimilative' illusion, the smaller and larger arcs are referred to the same centre.

(xx.) The lower trapezoid in *A* appears the smaller. The condition of the illusion is the overestimation of small angles. Suppose that the lines of fixation *cg*, *ae*, *bf*, *dh* are drawn. We shall then have at *a* and *e* acute angles below the parallels *ab*, *ef*; and at *c* and *g* acute angles above the parallels *cd*, *gh*. But this means that *ae* and *cg*, which are really parallel, appear to diverge above: hence the lower figure must appear the smaller.

In *B* we have this same variable illusion of direction, but we have in addition the 'assimilative' illusion of reference to a common centre. The total illusory effect is thus increased.

Neither *A* nor *B* gives rise to illusion if the figures are shifted from their direct vertical coincidence.

Questions.—(1) Retinal image: distortion of the image by dioptrical defects; phenomena of irradiation. Eye-movements: strabism; paralysis of eye muscles. Wundt, Täuschungen, 170 f.

(2) Constant illusions of extent and direction: purely physiological. Illusions of reversible perspective; variable illusions of

extent and direction: mixed. Illusions of association: purely psychological. Wundt, Täuschungen, 173.

(3) The answer to this Question is important for the 'proximate' understanding of Wundt's theory (p. 309 above). See Täuschungen, 172; Phys. Psych., ii., 439; *cf.* Titchener, Outline, 203.

(4) There are three: the requirement of a determinate position of the retinal image (more exactly, of a determinate direction of the lines of fixation); the rule that the point first fixated, and the point from which movement proceeds, appear nearer to the observing eye; and the rule that the perspective which corresponds to the usual conditions of tridimensional vision usually evokes the eye-movements that accord with it, and so is most frequently perceived. Wundt, Täuschungen, 171 f.

(5) This question is intended to introduce the student to Lipps' method of analysis. The answer should be worked out from the Raumaesthetik, 271, last paragraph. Lipps' reasoning is, at best, not easy to follow. It is, however, important that the student should have some knowledge of the mechanical-æsthetic theory, and give it the respect due to an acute and consistently worked out hypothesis. The method of appeal to particular figures, with comparison of explanations, has not been found satisfactory by the author; Lipps' arguments lose very considerably by separation from their context.

(6) This Question may be somewhat beyond the capacity of the average student. If it is attempted, its answer should be worked out from Wundt, Täuschungen, 157 ff.; Philos. Studien, xiv., 27 ff.; Witasek, Zeits. f. Psych., xix., 81 ff.; Lipps, Raumaesthetik, 1–69, esp. 61 ff. The Question gives the Instructor a good opportunity to emphasise, and by reference to historical conditions to explain, the 'intellectualistic' or 'logical' tendencies of popular psychology. See Külpe, Outlines, 189 ff.

(7) Fig. 49 (Pt. i.) shows a number of illusions of reversible perspective; Fig. 50, some variable illusions of extent; Fig. 51, variable illusions of direction; Fig. 52, illusions that may be regarded as associative; and Fig. 53, illusions that appear to be due to a complication of conditions. Many of these figures will be found in Sanford, Course, 212 ff.

§ 50. **Explanations of the Müller-Lyer (Arrow Head and Feather) Illusion.** —(1) *Delbœuf's Theory of 'Attraction of Regard.'* —Delbœuf considered that here, and in a number of other illusion-figures, the eye is drawn or attracted to or from the principal line by neighbouring lines. In Fig. 36 *A* (Pt. i.) the regard is attracted towards the central line in *a*, and away from it in *b*; *a* is therefore shorter, *b* longer. Delbœuf employed Fig. 84 to substantiate his theory. The illusion is much more pronounced in *A* than in *B*; the acute angles act more powerfully than the obtuse in drawing the eye to or from the principal line.

Revue scientifique, li., 1893, 237–241. Criticised by Brentano, Zeits. f. Psych., vi., 1–7; Einthoven, Pflüger's Arch., lxxi., 5; Heymans, Zeits. f. Psych., ix., 246 ff.; Thiéry, Phil. Stud., xii., 92; Wundt, Täuschungen, 93.

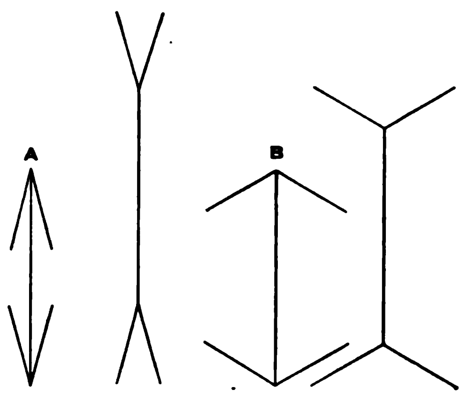


Fig. 84.—Delbœuf.

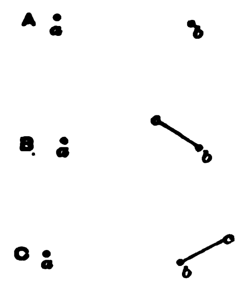


Fig. 85.—Brentano.

(2) *Brentano's Theory of tne 'Pseudoscopic Angle.'* —Brentano reduces the figure to its simplest form, Fig. 85. There is nothing in *B* to make the distance from the point to the end of the line seem shorter than the distance between the points in *A*, or than the distance marked off in *C*, except our estimation of the angle formed by the imaginary line *ab* with the line *bc*. We overestimate acute, and underestimate obtuse angles. The result of this tendency is shown in Fig. 86. The relation between the end-point of the line and the isolated point is here changed by our incorrect estimation of the angle, and the change of relation

directly affects our further estimation of distance, — underestimation of obtuse angles increasing, and overestimation of acute angles lessening the distance. In Fig. 87, the conditions of illusion are multiplied, and the illusion is therefore enhanced. In Fig. 88 the illusion is greatly diminished; in Fig. 89, it is practically destroyed.

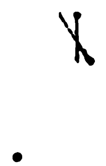
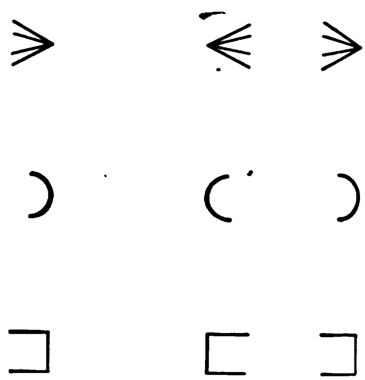


FIG. 86. — Brentano.



FIGS. 87, 88, 89. — Brentano.

Zeits. f. Psych., iii., 349–358 (esp. 356–8); v., 61–82 (esp. 77–82); vi., 1–7. Criticised by Delbœuf, Rev. sci., li., 237 ff.; Heymans, Zeits. f. Psych., ix., 236 ff.; Lipps, Zeits. f. Psych., iii., 498–504; Müller-Lyer, Zeits. f. Psych., ix., 7 f.; Thiéry, Phil. Stud., xii., 89.

(3) *Auerbach's 'Physiological' (Indirect Vision) Theory.* — In estimating the length of the divided line *ac*, Fig. 90, the eye in-

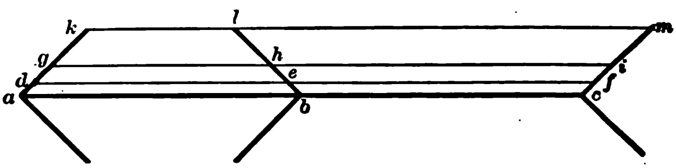


FIG. 90. — Auerbach.

voluntarily draws lines *df*, *gi*, *km*, parallel to *ac*. The line *ac* is bisected at *b*; but the line *bl* divides the three parallels unequally at *e*, *h*, *l*. This unequal division of the surfaces above and below *ac* affects our estimate of the divisions of *ac* itself. Hence *ab* becomes smaller than *bc*.

Zeits. f. Psych., vii., 152–160 (esp. 153). Criticised by Einthoven, Pflüger's Arch., lxxi., 5; Heymans, Zeits. f. Psych., ix., 236 ff.; Thiéry, Phil. Stud., xii., 88 f.

(4) *Brunot's 'Mean Distance' Theory.* — Our estimate of the comparative length of *ab* and *cd* in Fig. 91 is based not upon the

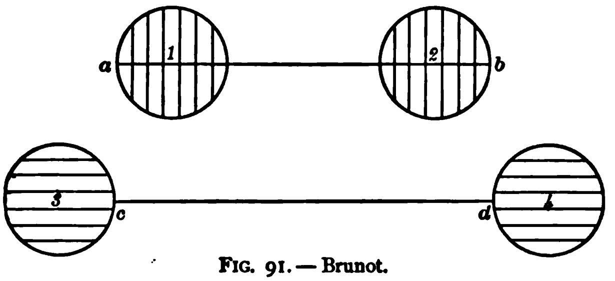
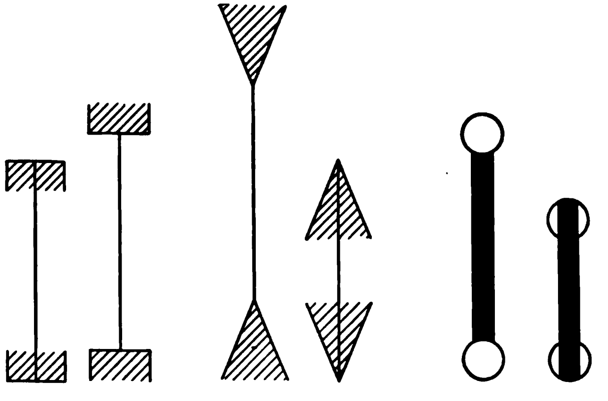


FIG. 91. — Brunot.

apparent lengths of the lines themselves, but upon the distances between the 'centres of gravity' of the spaces included by the terminal circles. We are, therefore, really comparing the lines *12*, *34*. The theory is substantiated by Figs. 92–94. In Fig. 94 the lines are rendered unusually important, and the influence of the end-spaces is reduced; in Figs. 92 and 93 the end-spaces are emphasised.

Rev. sci., lii., 212. Criticised by Thiéry, Phil. Stud., xii., 88.



FIGS. 92, 93, 94. — Brunot.

(5) *Müller-Lyer's 'Confluence' Theory.* — When two mental processes are set up by neighbouring stimuli, they may influence each other in the direction of greater likeness (confluence) or of

exaggerated difference (contrast). In the estimation of the equal lines of the figure we take account not only of the lines themselves, but also, involuntarily, of the spaces included by the oblique pieces. The line bounded by obtuse angles is therefore longer than that bounded by acute angles: in each case, the impression is strengthened by the accompanying impressions, and strengthened in the direction of these latter (confluence). — The explanation is not unlike that of Auerbach, though the principle upon which it rests is different.

Du Bois-Reymond's Archiv, 1889, S. B., 266 f.; Zeits. f. Psych., ix., 1-16 (esp. 4 ff.); x., 421. Criticised by Einthoven, Pflüger's Arch., lxxi., 4 f.: Heymans, Zeits. f. Psych., ix., 236 ff.; Láska, Du Bois-Reymond's Arch., 1890, 326-328; Thiéry, Phil. Stud., xii., 88; Wundt, Täuschungen, 91 ff.

(6) *Thiéry's 'Perspective' Theory.* — The line that appears nearer is seen as smaller, the line that appears farther off is seen as longer. If the central point of Hering's figure (Pt. i., Fig. 39 *A*) be drawn out, in imagination, to form a horizontal line, it will represent the (apparently) longer Müller-Lyer line, and be seen behind the plane of the paper. If a line be drawn in Wundt's figure (Pt. i., Fig. 39 *B*) between any corresponding pair of angles in the centre of the diagram, it will represent the (apparently) shorter Müller-Lyer line, and be seen before the plane of the paper. The illusion of irreversible perspective in the Müller-Lyer figure is, as we said above (p. 314), that the shorter line seems to be nearer than the longer. Thiéry makes the difference of distance the primary illusion.

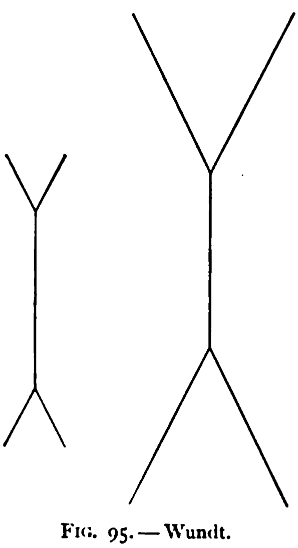


FIG. 95. — Wundt.

Phil. Stud., xii., 73 ff. Criticised by Wundt, Täuschungen, 101.

(7) *Wundt's 'Eye-movement' Theory.* — Every line of fixation is traversed by means of a determinate movement, or (if the eye remain at

rest) contains a 'motive' to the execution of such a determinate movement. Eye-movements may be free, continuous, or may be hampered, arrested; and the difference is reflected in our perceptions of spatial extent. Lines containing a motive to the continuance of movement in their own direction are overestimated; lines containing a motive to the arrest of movement are underestimated. The illusion is, therefore, lessened in the longer figure of Fig. 95, where the oblique pieces lie farther out of the direction of the vertical than they do in the shorter figure.

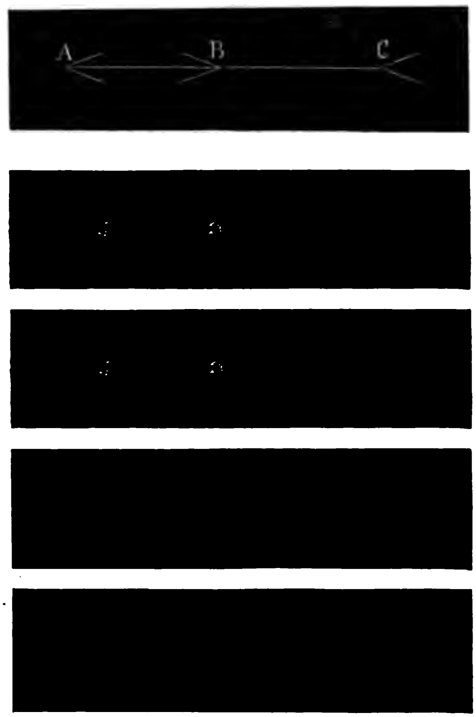


FIG. 96. — Einthoven.

Täuschungen, 100 ff. Criticised by Einthoven, Pflüger's Arch., lxxi., 5; Heymans, Zeits. f. Psych., ix., 246 ff.; Müller-Lyer, Zeits. f. Psych., ix., 9.

(8) *Einthoven's 'Dispersion Image' Theory.* — The only parts of the figure clearly seen are the parts directly seen. Fig. 96 shows how the parts indirectly seen may be changed by dispersion.

Pflüger's Arch., lxxi., 1 ff.

(9) *Láska's Theory of 'Joining the Discontinuous.'* — All our judgments are the resultant of two sets of factors: habitual tendency and present circumstances. In this case, our tendency is to join a discontinuous figure, to make it continuous, by the shortest possible road; the figures themselves supply the circumstances. But the lines necessary to make a complete figure of the pointed line are shorter than those required to make a

complete figure of the feathered line; therefore the latter is the longer.

Du Bois-Reymond's Arch., 1890, 326. Note that Láska himself does not make the above definite application of the theory; he merely throws it out in general terms. The explanation resembles that of Auerbach.

(10) *Heymans' 'Movement Contrast' Theory.*—When one fixates the end-point of either of the vertical lines, the eye takes in the oblique pieces as well. This perception implies an idea of eye-movement, from the point of rest to the end-points of the oblique pieces. And this, in turn, implies a very strong tendency to actual movement. Now in the case of the pointed vertical, the suggested movement is in a similar direction to movement along the principal line; in the case of the feathered vertical, it is in a different direction. Hence, in the former case, the sum of eye-movement is less than it is in the latter: just as a foregoing red weakens a following red but strengthens a green. We have, accordingly, the underestimation and overestimation of the two verticals. Contrast occurs in two forms: the inward and outward directions of the oblique pieces exert opposite influences upon the movement of regard; and, within each figure, the upper and lower oblique pieces have an opposite effect. The former statement needs no further explanation; the latter is explained as follows. Increase of the illusion depends upon the oblique pieces with which the eye-movement begins; its decrease depends upon those with which the movement ends. For, if the obliques are lengthened, the influence of the initial pair is gradually lessened, and finally altogether destroyed: this is, of course, fully demonstrable only on the feathered vertical, since only there can the obliques be increased to any required length. The reason is, that the initial obliques exercise their full power only when they are relatively short, and so directly seen; as they lengthen, they are seen less and less clearly, and their superiority (as illusion-producing factors) over the final obliques (the illusion-arresting factors) is presently reduced to zero.

Zeits. f. Psych., ix., 248–252. Criticised by Einthoven, Pflüger's Arch., lxxi., 5; Wundt, Täuschungen, 92–94, 160–162.

(11) *Lipps' 'Mechanical-æsthetic' Theory.*—This explanation

is, unquestionably, that which suffers most by disjunction from its context. It shall be given, so far as possible, in Lipps' own words. Why do we overestimate the length of the line (*e.g.*, of a horizontal line) that lies between diverging end-pieces? The answer is this. "The terminal points of the line limit it, and limit also the oblique pieces. This second limiting activity works upwards and downwards, in so far as the oblique pieces trend upwards and downwards. It works outwards, in the horizontal direction, in so far as the oblique pieces trend outwards. Just in so far as this is the case must the limiting activity of the terminal points towards the inside, towards the horizontal line itself, be cancelled by their limiting activity outwards, towards the oblique pieces. To the same degree, of course, the limiting activity of the points towards the oblique pieces is cancelled by their activity on the side of the horizontal line. That is to say: the length of all these lines is overestimated." Why, on the other hand, do we underestimate the length of the horizontal line that lies between converging end-pieces? "The limiting activity which the common terminal point exerts upon the horizontal line, and the limiting activities, relatively of the same direction, which it exerts upon the oblique pieces, reinforce one another." The illusion is less than in the former case, because the tension (produced as reaction by the increase of the limiting activity) is greater.

Raumaesthetik, 237, 250; Zeits. f. Psych., iii., 502 f. Criticised by Brentano, Zeits. f. Psych., v., 77–82; Einthoven, Pflüger's Arch., lxxi., 5; Heymans, Zeits. f. Psych., ix., 243 ff.; Wundt, Täuschungen, 164 (general criticism).

(12) *Jastrow's 'Relativity' Theory.*—Jastrow does not deal directly with the Müller-Lyer illusion, but lays down general principles upon which this and the cognate illusions may be explained. They are all, he thinks, essentially psychological in origin, illusions of judgment and not of perception. And they are all reducible to the law that we are prone to judge relatively, *i.e.*, to modify our judgment according to environment.

All angles are overestimated. If we call "the direction of an angle the direction of the line that bisects it and is pointed toward the apex, then the direction of the sides of an angle will

be deviated toward the direction of the angle." When obtuse and acute angles are so placed as to lead to opposite deviations, the former will outweigh the latter, and the illusion will appear according to the direction of the obtuse angle. The smaller the angle, the less is the illusion. And "just as the presence of angles modifies our judgment of the directions of their sides, so too, the angles will modify the apparent lengths of lines."

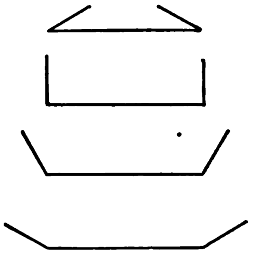


Fig. 97.—Jastrow.

Amer. Journ. of Psych., iv., 1891–2, 381 (382, 396).

The following analysis may save labour in the use of the bibliography.

(1) Delbœuf (Rev. sci., li., 237) criticises Brentano.

(2) Brentano (Zeits. f. Psych., v., 77–82) criticises Lipps; (vi., 1) criticises Delbœuf.

(3) Auerbach (Zeits. f. Psych., vii., 1894. 152) criticises Brentano.

(5) Müller-Lyer (Zeits. f. Psych., ix., 6–16) criticises Auerbach, Brentano, Delbœuf, Láska, Lipps, Wundt.

(6) Thiéry (Phil. Stud., xii., 87–94) criticises Auerbach, Brentano, Brunot, Delbœuf, Müller-Lyer.

(7) Wundt criticises Delbœuf, Heymans, Müller-Lyer, Thiéry.

(8) Einthoven criticises (very hastily) Auerbach, Brentano, Delbœuf, Heymans, Lipps. Müller-Lyer, Wundt.

(9) Láska (Du Bois-Reymond's Arch., 1890, 326) criticises Müller-Lyer.

(10) Heymans (Zeits. f. Psych., ix., 236–248) criticises Auerbach, Brentano, Delbœuf, Lipps, Müller-Lyer, Wundt.

(11) Lipps (Zeits. f. Psych., iii., 498) criticises Brentano.

A good summary of the views of Thiéry, Müller-Lyer, Heymans, Lipps and Burmester is given by V. Henri, in L'Année psychologique, iii., 1896, 495. The views of Heymans, Lipps and Wundt are summarised *ibid.*, iv., 1897, 538.

CHAPTER X

Auditory Perception

EXPERIMENT XXX

§ 51. **Tonal Fusion.** — The doctrine of tonal fusion is very far from being a closed chapter. Indeed, one of the most recent writers on the subject, E. Buch, denies that there is any need of the term 'fusion' at all, and professes to reduce the phenomena of fusion to the ordinary laws of association (Philos. Studien, xv., 1900, 268; *cf.* M. Meyer, Zeits. f. Psych., xxii., 1900, 460). It is necessary, then, at the outset, to have a clear definition of the word.

"If two tones whose pitch-numbers stand in the ratio 1:2 are sounded together, they can be but very imperfectly separated (*gesondert*) as compared with two tones, given under the same conditions, whose pitch-numbers form the ratio 40:77" (Stumpf). This difficulty of separation depends upon "an invariable peculiarity of the sensation-material, which persists when all other obstacles to analysis have been removed." In the one case the tones 'come apart' in sensation; in the other they form a whole or total impression, nearly akin to the impression of the simple sensation. Fusion is, then, a phenomenon of sensation, a *sinnliches Phänomen*, not an hypothesis set up to explain the problems of tonal mixture.

Stumpf defines fusion as "that relation of two sensation contents in which they form not a sum but a whole"; "that relation of two sensations, in consequence of which (in its higher stages) the total impression approaches more and more closely to that of a single sensation, and is analysed with greater and greater difficulty." The reader must be careful not to misunderstand these statements. A definition is necessarily couched in logical terms, and there is a temptation to translate the terms

into psychological processes. But the fusion-relation is not something superadded upon the tone sensations. Two tones, sounded together, are given-fused, given in the relation of fusion; the fusion is the sound of the tones as they sound together. Nor must any process-meaning be read into the word 'fusion.' There is no trace of fusing, of being fused, when the tones sound. They form a blend,—as we speak of a 'blend of tea'; and this blend, the sound-whole, is the fusion.

Stumpf gives two further cautions. The fusion of two tones is not identical (even at its highest degree) with the origination of a third tone. And we must be on our guard against spatial metaphors. "All spatial extension is either outside or identical. But simultaneous tones offer an instance of interpenetration (*Durchdringung*)."—See Tonps., ii., 64 f., 127 ff.; Zeits. f. Psych., xv., 280; xvii., 422; Beitr. i., 42: *ct.* Meyer, Zeits. f. Psych., xvii., 414; xx., 1899, 28.

MATERIALS.—The harmonicas can be bought for about 25¢ each. Each instrument has twenty reeds, ten of which speak to inspiration, and ten to expiration. It is the latter ten only that are used in the Experiment. The blowing-device described in the text may be improved by having, instead of the quills, two sliders of sheet tin, fitting over the top of the harmonicas, and carrying a short piece of tin tube, over which the rubber tubing can be slipped.

It need hardly be said that the harmonicas are recommended simply on account of their extreme cheapness. They should be selected from the music-dealer's stock as carefully as the cheap forks of Exp. V. In default of better instruments, they 'work' very well.

For more elaborate experiments, the reed-clangs of the harmonical and the tones of tuning forks may be recommended. The tests reported in the literature have been carried out upon organ-pipes, church organ, piano, violin, forks, etc.—On the use of clangs and tones, see Stumpf, Zeits. f. Psych., xvii., 1898, 423 ff.; Meyer, *ibid.*, 412.

EXPERIMENT.—The method indicated in the text is that to be followed with *unmusical* observers. There is no direct estimation of fusion degree; the scale is constructed from the re-

sults, by inference, after a sufficient number of trials has been made to exclude chance errors. The 25-limit is theoretically too low; but in practice, with careful observation should be adequate to the purpose of the experiment. The time available for a single exercise is so short that full series can hardly be taken. Even as it is, the Instructor will probably do well to assign several *O*'s (separated from each other by cardboard screens, arranged along the table at which they sit) to a single *E*.

The single-note stimuli must be introduced here and there into the interval-series, in order to prevent bias upon the part of *O*. If one knows that two notes are to be given, one will naturally tend to hear two notes in every case. The single-note judgments may be neglected in the final calculation, and the tests are not included in the 300 total. Doubtful judgments may be counted ½ to the right ('two note') and ½ to the wrong ('one note') judgments. Doubtful judgments with inclination in a particular direction may be counted (though this is rough procedure) ¾ to the judgments towards which they incline, and ¼ to those of the opposite class.

There are five principal cautions to be given with regard to this experiment. (1) The number-differences upon which the scale of fusion degrees is based must be considerable, and, if not absolutely uniform, still extremely constant. (2) If a wide interval give more correct judgments than a narrow interval, the latter is not necessarily the better fusion; the judgments may be based upon distance in the tonal scale. But if a narrow interval give more correct judgments than a wide interval, the latter is very certainly the better fusion. (3) Pleasantness and unpleasantness of impression must not be confused with higher and lower degree of fusion. (4) All the tones of a series must be sounded with approximately the same subjective intensity. (5) Each pair of tones must begin and end at the same moment and in the same way. Extreme accuracy of intonation is, fortunately, a matter of indifference with unmusical observers.

With musical observers a different procedure must be followed. Such observers are able to pass *direct* judgment upon fusion degree. There are two chief obstacles to the analysis of

a sound-complex by an unmusical ear: lack of practice, and fusion. With musical observers, the first of these obstacles is removed. In their case, therefore, "as soon as analysis has been performed, and the tones clearly cognised as two, the fusion can be remarked for itself" (Stumpf).

The method is that of 'paired comparisons' (see Exp. XXI.). Every interval is compared with every other interval as regards degree of unitariness. A scale of degrees will be obtained, which tallies in all essentials with the scale of indirect (unmusical) observation.

Points to be noted are the following. (1) The degree of certainty with which a given interval is adjudged a better or worse fusion than another interval should be carefully set down. For this direct scale of fusion degrees depends, not upon the number of tests made, but upon the conviction of the trained observer. (2) Distance upon the scale must, again, be distinguished from degree of fusion. (3) The observer must be upon his guard against the confusion of pleasantness or unpleasantness of the impression with the sensible relation (fusion degree) obtaining between its component sensations. (4) The beginning, ending and subjective intensity of the tones must be regulated as before. (5) The more delicate the ear, the more accurate must be the intonation of the intervals. (6) The observer must abstract from his knowledge of the musical significance and rating of the clangs submitted to him for analysis, and from all considerations of harmony and inharmony, direct and indirect relationship, etc.

On the technique of work with forks, see Wundt, Phys. Psych., 1893, i., 460; R. Schulze, Philos. Studien, xiv., 1898, 473.

On method in general, see Stumpf, Tonpsychologie, ii., 140 ff.; Zeits. f. Psych., xv., 1897, 297 ff.

RESULTS. — The percentage of errors (*i.e.*, of judgments of unity when two notes are sounded) with unmusical observers will be roughly as follows:

Octave	75 %.
Fifth	40 to 60 %.
Fourth	28 to 36 %.
Thirds and sixths	20 to 30 %.
Seconds and sevenths	ca. 15 %.

The tritone will, in all probability, fall between the fourth and fifth groups, rather nearer the former than the latter: its position cannot, however, be predicted as certainly as can those of the other intervals. The wide range of the percentages is due (apart from aids or hindrances to analysis inherent in the use of a particular instrument, etc.) to the indefiniteness of the term 'unmusical.' There are all degrees of transition between 'musical' and 'unmusical' observers; and though the extremes themselves are well marked, they are certainly not absolute. The Instructor must, therefore, inform himself as accurately as he can of the status of his observers in musical regard; and the principal data upon which the characterisations 'altogether unmusical,' 'distinctly unmusical,' etc., are based should be entered in the student's note-book. — See Stumpf, Zeits. f. Psych., xv., 299; Tonpsychologie, ii., 142 ff.

Stumpf's tests for unmusicalness are as follows. (*a*) A note within the compass of the observer's voice is struck upon the piano, and he is then required to sing it. (*b*) Two successive notes are played, and he is required to say which is the higher of the two. (*c*) Intervals are played (high and low fusion degrees in random order), and he is asked in each case whether he has heard one note or two. (*d*) Two chords are played, in fairly quick succession, and he is asked which is the pleasanter or less pleasant of the two. — Tonpsychologie, ii., 157 ff. These tests may be supplemented by suitably prepared questions, regarding the observer's musical training in childhood, his interest in music, his ability to recognise and 'carry' an air, etc.

On the scale of fusion degrees, in direct and indirect observation, see Stumpf, Tonpsychologie, ii., 135, 142 ff.; Külpe, Outlines, 286; A. Faist, Zeits. f. Psych., xv., 1897, 102; A. Meinong and S. Witasek, *ibid.*, 189; M. Meyer, *ibid.*, xvii., 1898, 401; E. Buch, Philos. Studien, xv., 1900, 1, 183.

QUESTIONS AND COGNATE EXPERIMENTS. — (1) There are five or six degrees of fusion: five, if we count the tritone with the group of thirds and sixths, or with the group of seconds and sevenths; six, if we regard the tritone as a transition interval between these 'imperfect consonances' and the 'dissonances.' It is probable that the latter view is the more correct: that there is a 'seven-group,' consisting of the intervals 4 : 7 (natural seventh) and 5 : 7 (approximate tritone), roughly represented in

our experiment by the tritone 32 : 45, which lies between the group of thirds and the group of seconds.

There is a greater difference between the better than there is between the worse fusions. This fact is brought out by the diagram (modified from Stumpf), in which the higher wave-crests stand for the higher degrees of fusion, and the base-line gives the fusion degree of the dissonances.—Stumpf, Tonps., ii., 135, 176; Zeits. f. Psych., xv., 284; Faist, *ibid.*, 114.

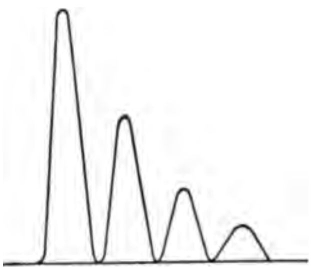


FIG. 98.—The fusion relation of octave, fifth, fourth, and thirds and sixths.

(2) "The degree of fusion is a function of the vibration-ratio of the component tones."—Stumpf, Tonps., ii., 136; Zeits. f. Psych., xv., 287; Külpe, Outlines, 286; Faist, Zeits. f. Psych., xv., 121.

(3) In general, the degree of fusion is independent of the tone region. This law may be tested over the three octaves of the harmonicas. In the very lowest part of the scale, where analysis is difficult or impossible, the recognition of fusion degree becomes difficult and impossible. And in the highest part of the scale, from tones of about 4000 vs. onwards, the fusion differences disappear. Elsewhere they persist unchanged.—Stumpf, Tonps., ii., 136; Külpe, Outlines, 286; Faist, Zeits. f. Psych., xv., 122.

(4) Fusion degree is independent of the absolute intensity of the tones. So long as these can be heard at all, and so long as they are not too loud to overpower the organ, the degree of fusion remains unchanged. Stumpf holds the same opinion with regard to the relative intensity of the tones. Provided that the weaker tone is audible at all in the sound-complex, the degree of fusion remains unchanged. Külpe, on the contrary, argues that the weaker tone, if the weakening be carried far enough, becomes a mere attribute or 'colour' of the stronger, so that the total impression is made more unitary. The difference of opinion seems to be due to a confusion, on Külpe's part, of 'fusion degree' and 'ease of analysis.' There can be no doubt that variation of the relative intensity of a component tone will hinder or facilitate

the analysis of the tonal perception. But this simply means that fusion degree and relative intensity are coördinate factors in analysis; it does not mean that alteration of relative intensity effects an alteration in fusion degree. The latter persists, after analysis, precisely as it was before. Stumpf asks the pertinent question: "How do we recognise the octave, if not by its fusion? And do we not recognise the octave when one of its tones is weaker than the other?"—Tonps., ii., 136; Zeits. f. Psych., xv., 288; Külpe, Outlines, 288; Faist, Zeits. f. Psych., xv., 124.

(5) Stumpf maintains that the fusion degrees beyond the octave are the same as those within the octave. "The ninths have the same fusion as the seconds, the tenths as the thirds, the double octave and triple octave as the octave, and in general $m : n \cdot 2^x$ the same as $m : n$, where $m < n$, and x is a (small) whole number." Külpe asserts, on the contrary, that "while the relative degree of fusion remains the same for intervals beyond the octave that it is for corresponding connections within the octave, all the intervals of the former kind stand upon a somewhat lower level of fusion than their less remote correlates. In other words, the double octave possesses a higher degree of fusion than the twelfth, the twelfth than the tenth, etc., but the double octave in its turn fuses less well than the octave, the twelfth than the fifth, etc."

Stumpf replies (*a*) that the observer must not be misled by greater ease of analysis. Distance upon the scale may help analysis, while it still does not affect fusion degree. He asks, further, (*b*) how it is that we recognise the double octave, except by the fact that the two tones have the same fusion as those of the octave, and merely lie a greater distance apart. Finally, he urges (*c*) that the alleged decrease of fusion degree must either be so small as to be negligible in comparison with the stages of fusion within the octave, or must be so great that a consonant interval ultimately passes into a dissonance.

Passing over certain other and less important arguments, we may meet these three as follows. (*a*) Let the observer compare the intervals of the fifth and the twelfth, with analysis in each case, so that he can direct his attention upon fusion degree and upon it alone. He will find that the twelfth-tones come apart

in perception more easily than the fifth-tones. (*b*) The fact that we recognise the double octave as the ***double*** octave tells against Stumpf. We do not recognise the twelfth as the fifth, but as the twelfth; we do not recognise the ninth as the second, but as the ninth. There is a sense-difference present, over and above the difference of tonal distance. (*c*) The third argument has weight only on the assumption that the scale of fusion degrees must be a linear scale. There is, however, no reason why there should not be a second linear scale, parallel with the intra-octave scale, the steps upon which agree in relative position with the steps within the octave, but have their places in a different fusion system.

While, therefore, the author agrees with Stumpf on (4), he agrees with Külpe on (5). These judgments are the outcome of more or less systematic work (direct observation; König forks and Ellis harmonical) repeated every year since 1892.

Stumpf, Tonps., ii., 139; Zeits. f. Psych., xv., 293; Külpe, Outlines, 287; Faist, Zeits. f. Psych., 130.

(6) This question includes the question of the influence of clang-tint upon degree of fusion. On the negative side, see Stumpf, Tonps., ii., 136 (*cf.*, however, Beitr., ii., 1898, 168); Zeits. f. Psych., xv., 290; Faist, *ibid.*, 127. On the positive, Külpe, Outlines, 293; Faist, Zeits. f. Psych., xv., 128; Meyer, *ibid.*, xvii., 413 ff.; xviii., 1898, 274 ff.; xx., 1899, 445. We have no alternative but to suspend judgment, until more systematic experiments have been made.

(7) The degree of fusion remains unchanged, although analysis is facilitated.—Stumpf, Tonps., ii., 138; Zeits. f. Psych., xvii., 423; Külpe, Outlines, 299.

(8) No; although, *e.g.*, the beats which the actual tones would make can be slowed, quickened, or entirely suppressed in ideation.—Stumpf, Tonps., ii., 138; Beitr., i., 10 f.; Faist, Zeits. f. Psych., xv., 130.

(9) We have already had: distance on the tonal scale, absolute and relative intensity, spatial separation. To these may be added: number of simultaneous tones, duration of the clang, partial tone change (qualitative or intensive); attention, practice and fatigue, expectation and habituation, memory.—Stumpf,

Tonps., ii., 318 ff.; Külpe, Outlines, 298 ff.; Sanford, 72, exp. 84.

(10) We must make a distinction between analysed and unanalysed clangs. (*a*) Analysed clangs. "In a continuously sounding compound clang, the whole appears to possess the pitch of its deepest tone, even if this be not the loudest" (Stumpf). "The rule holds, in the author's experience, only for stimulation by clangs containing numerous overtones" (Külpe). —Tonps., ii., 384; Outlines, 304; Sanford, 72, exp. 85. (*b*) Unanalysed clangs. *i.* Unequal intensity of the components. Here we find two illusions: a simple clang appears a little lower than the compound clang of equal fundamental pitch-number with which we compare it; and the absolute pitch of a simple clang may be estimated one or two octaves too low. *ii.* Equal intensity of the components. Here it depends upon circumstances whether the higher or the lower tone shall 'carry' the whole sound-mass.—Tonps., ii., 406, 410.

The answers here given are, of course, merely schematic. It must be left to the discretion of the Instructor whether or not he carry the doctrine of tonal fusion into greater detail; and it must be left to his ingenuity to devise or select experimental tests of the various 'laws' of fusion. One of these laws (that which deals with the effect of mistuning upon degree of fusion: Stumpf, Tonps., ii., 137; Zeits. f. Psych., xv., 288; Faist, *ibid.*, 129) has been left without mention in the text; and many interesting points (the apparent interval between simultaneous tones, the movement of the tonal mass in a succession of chords, etc.) have also been passed over. A student who has the desire and the ability to gain a 'judgment' in matters of qualitative tone-psychology cannot begin better than by reading through Stumpf's second volume, abstracting as he goes, and working out the illustrations (so far as possible) with the instruments and under the conditions prescribed by the author.

EXPERIMENT XXXI

§ 52. **Rhythm.**—The best introduction to the psychology of rhythm is to be found in ch. i. of E. Meumann's Untersuchungen zur Psychologie und Aesthetik des Rhythmus, Pt. i., Philos.

Studien, x., 1894, 249. The problem is clearly formulated on p. 273: "What we now need most of all are an exhaustive introspective description of those data of the inner experience which we term specifically 'rhythmical'; the reference of these experiences to the operation of mental factors of general validity; and the determination of the conditions under which the specifically 'rhythmical' impression arises." We have, in other words, to describe and explain the perception of rhythm, and to give it its place in our system of psychology. Although no one of these three part-problems has as yet been fully solved, the very fact of their discrimination and definite formulation marks a great step in advance. Where we have a problem and a method, it becomes merely a matter of time until we also have the answer.

On temporal ideas in general, see Wundt, Outlines of Psych., trs. 1897, pp. 142–158. On the advisability of beginning work with uniform sounds, see Meumann, 302; Bolton, Amer. Journ. of Psych., vi., 1893, 178. On visual rhythm and auditory symmetry, see Meumann, 279; Philos. Studien, x., 1896, 261; M. K. Smith, *ibid.*, xvi., 1900, 288, 296, 299.

MATERIALS. — It is said in the text that the variation of auditory stimuli is easily regulated. This is principally due to the fact that the rise and fall of auditory sensations are exceedingly short, "so that any temporal succession of sounds is reproduced with almost perfect fidelity in the corresponding succession of sensations" (Wundt). On the other hand, the instruments which give the rhythm stimuli, the variously modified sound series, must have a high degree of mechanical accuracy, and are correspondingly expensive. The most useful appliance for investigation is, probably, Meumann's 'time-sense' apparatus, consisting of Baltzar kymograph, time-disc, set of contacts, and sound-hammers (described and figured in Philos. Stud., ix., 1894, 270 ff.; xi., 1896, 145 ff.). This, however, is not to be thought of for a drill-course. Bolton's apparatus (Amer. Journ. of Psych., vi., 1893, 179 ff.) might be simplified; but the author has had no opportunity of working with it.

The metronome recommends itself, for the initial experiment, both by its cheapness and by its wide range of rate. Unfortu-

nately it is true in most cases, as Bolton says, that "the two sounds heard during a complete swing of the pendulum of the metronome are not of the same intensity or quality" (*op. cit.*, 205 f.; *cf.* F. Schumann, Zeits. f. Psych., i., 1890, 77). The Instructor should, therefore, select the instrument himself from the music-dealer's stock, and not order at haphazard. There is a great difference between one metronome and another, despite the sameness of make. An instrument whose clacks sound approximately equal on the music-counter (and such an one should be found among the first half dozen tried) will give still more nearly equal ticks in the laboratory, when resonance is ruled out. With a metronome so chosen there is no "impossibility of restraining the grouping by two." As will be seen from the Results, *O* may give 3-groups, or no group at all. And where this happens, we may be sure that the differences in intensity, quality and clangtint, which we have intended to eliminate, *are* so far eliminated that they have become subliminal, and do not influence *O*'s attitude to the sound series.

It should, perhaps, be said that the author has not been able to find a bell-metronome which answers the purpose of this first experiment. This is regrettable, as the bell-metronome is required for Exp. XXV. above.

Preliminaries. — The use of six rates is arbitrary: more may be employed, if time permits. The metronome is, in general, a very reliable instrument. Nevertheless, these rates should be tested, on principle, by counting the beats with a stop-watch.

Experiment (1). — The object of this experiment is to bring out the fact of subjective rhythmisation, or (as it is also termed) subjective accentuation. Most *O*'s (there are exceptions) cannot listen to a sound series without, so to speak, hearing a rhythm into it. In some cases this subjective rhythmisation rises to the height of pure illusion; *O* insists that the sounds differ considerably in objective intensity (Bolton, 192, 195, 200, 202; Meumann, 302).

It is evident that suggestion to *O* must, as far as possible, be avoided. The author would, therefore, advise that two students make their preparations with two metronomes, working together, and then that two others, who know nothing of the experiment,

be called upon unawares to serve as *O*. This is the only way in which the facts of involuntary (purely subjective) rhythm can be certainly secured.

Meumann (302) gives as the conditions of purely subjective rhythmisation: (1) a high rapidity of succession of the sound impressions (less than .4 sec. intervals); (2) absolute equality of impressions as regards intensity and quality; (3) a fairly long continuance of the sound series; and (4) a receptive and passive attitude to the sound series on the part of *O*. The statement as to rapidity of succession has led the author to select the two intervals .39 and .3 sec. for the first experiment, though he is by no means sure that the time limit is valid. It is true, as Meumann says, that Bolton, whose upper and lower limits are 0.1 and 1.5 sec. respectively, did not distinguish with sufficient care between involuntary and voluntary (suggested) rhythmisation. Nevertheless, the author has found cases of apparently pure subjective accentuation with a time interval of over 1 sec. between click and click.[1] The nature of the sounds employed, and individual differences of rhythmical disposition (of which we shall have evidence presently), may both exert an influence. The second and fourth conditions must be fulfilled to the letter. Any noticeable irregularity in the stimuli is disturbing; and separate attention to the sounds as such will prevent the formation of a rhythm (Bolton, 207). As for the third condition, the author has found 45 sec. to be the most satisfactory time for the average *O*. Rhythmically disposed *O*'s may not object to a longer series; but for the most part a prolongation of the clicks to 70 sec., even at fairly quick rates (.65 and .39 sec. intervals), will render *O* uneasy, and perhaps evoke the exclamation 'This is horrible!' or 'This is unbearable!' The 'listening' to a series of sounds, without the least hint of what is to be listened for, puts a severe strain upon the attention.

Full reports of 30 observers will be found in Bolton, 186 ff. The following Results were obtained without any the least suggestion that might direct *O*'s attention to subjective grouping.

[1] M. K. Smith (Philos. Studien, xvi., 1900, 282) places the lower limit of subjective sound-rhythmisation at "less than 2 sec." G. Martius asserts (*ibid.*, vi., 1891, 196) that the 'constraint' towards accentuation ceases at "less than 0.5 sec."

O (1). Slight musical training; not trained in introspection. Critical attitude to experiment; some amusement at the 'nonsense' of listening for no reason. Time of stimulus: 45 sec.

SERIES 1

EXP.	INTERVAL	GROUPING	REMARKS
1	1.4	No group	Beats made a *rat-tat*. Counted, to see if they were regular. Found them perfectly regular.
2	.9	"	Sound made me drowsy. Attention wandered.
3	.28	"	Beats resembled clock ticking. Tried to put poetry to them. Unpleasant.
4	1.2	"	Counted up to 50. Attention wandered.
5	.65	"	Noticed that beats were more rapid. Attention wandered.
6	.39	"	Clock movement again. Quite unpleasant sensation in head.
7	1.2	"	Sound suggested blacksmith's shop.
8	.39	"	Clock again. Annoyed by its rapidity and regularity.

SERIES 2

EXP.	INTERVAL	GROUPING	REMARKS
9	1.43	No group	Attention wandered.
10	.65	"	Suggested clock.
11	.3	"	Rapid clock ticks.
12	.9	"	Normal ticking of clock; breaking stone.
13	1.43	"	Pencil tapping on slate.
14	.65	"	Clock.
15	.3	"	Clock. Sounds seemed to move farther off at end of series.
16	1.2		No suggestion.

SERIES 3

EXP.	INTERVAL	GROUPING	REMARKS
17	.39	No group	Clock: also vague suggestion in the sounds.
18	.9	2-group?	Clock; head swayed backwards and forwards.
19	1.2	No group	Beats came at regular intervals, and all alike.
20	.3	2-group?	Regular at first; then the interval between the different sets of tick-tocks seemed longer than the interval between tick and tock.
21	1.4	No group	Beats regular.
22	.9	"	Beats regular at first; then grew confusing.
23	.39	2-group?	Beats went in couples. Interval between couples seemed longer; but by beating time I proved that all the intervals were equal.
24	.65	No group	Thought of making couples, but could not. Intervals between the beats were even.

It is noteworthy that this *O* did not once, in the 24 tests, notice any variation of intensity in the metronome sounds. The illusion of rhythm, where it begins to appear, is an illusion of time-interval, not of intensity. It was found impossible to induce subjective accentuation by suggestion. This *O* is therefore to be classed with Bolton's Subjects 18 and 30. — The results furnish good evidence of the steadiness of the metronome.

O (2). No musical training; not trained in introspection. Listened passively without criticism. Time of stimulus: 45 sec.

Series 1

Exp.	Interval	Grouping	Remarks
1	1.4	No group	Counted as high as 40. Some beats seemed louder than others.
2	.9	"	Counted a little; breathed deeply several times: beats seemed very slow.
3	.28	"	Saw telegraph key; then horses. Heard sound of rapid driving.
4	1.2	2-group	At once began beating time: one′, two. Saw soldiers marching.
5	.65	"	Could not count the beats, because they kept saying One′, two. The sound seemed to be hitting first on one side of the head and then on the other.
6	.39	"	Felt confused at first; sound seemed to knock me from side to side. Then the second beat seemed to be an echo of the first; it was broader but less intense.
7	1.2	"	Moved head backwards and forwards in time to beats. Uncomfortable twitching in eyes and lids until I beat time with my foot, when the twitching stopped.
8	.39	"	Whole body moved. Sounds seemed to say Get′ up, get′ up.

Series 2

Exp.	Interval	Grouping	Remarks
9	1.4	No group	Attention wandered. Wanted to nod my head, but thought it would look ridiculous.
10	.65	"	Thought of blacksmith and shoemaker. Felt as if I must keep my hands clasped, or in some way I should go to pieces.

Exp.	Interval	Grouping	Remarks
11	.3	No group	Saw two small sparks before my eyes. Eyelids twitched; felt frightened; could not breathe well; when metronome stopped, tears came.
12	.9	2-group	Beats said Read'y, read'y, and I could not keep quiet.
13	1.4	"	Driving nails. Saw house; nodded head; counted One', two.
14	.65	?	Attention distracted by noise in room. Head and body moved in time to beats; but felt confused.
15	.3	No group	Confused; head moved, eyes twitched. Thought of a railroad train.
16	1.2	2-group	Saw soldiers marching through a street. Then became confused, and wished beats would stop, I felt so uncomfortable. Soon I began to beat time with foot and head, and then I was sorry when beats stopped.

Series 3

Exp.	Interval	Grouping	Remarks
17	.39	2-group	Beats seemed to say Get' up, get' up. Very loud. Head wanted to move; arms jerked. Felt that if I stopped something would happen.
18	.9	"	Thought of Hickory dickory dock. Kept time with right foot and head; annoyed because they would not keep the same time.
19	1.2	"	Time seemed slow. Saw a see-saw. Confused till I counted One', two.
20	.3	4-group	Counted One', two, three', four, with a pause after the three'. Had heard a coach at a foot-ball game count in that way.
21	1.4	No group	Counted up to 36. Saw a long line of people going through a turn-stile. Thought of World's Fair.
22	.9	2-group	Counted One', two. Lost count and got confused; eyes twitched and I felt queer.
23	.39	4-group	First beats sounded like a giggle. Settled down into Ba', ba, ba', ba. Saw a clown, and wanted to laugh. Thought of "The Man who Laughs," and seemed to be following him. Beats became disagreeable, and pounded me. Glad to stop.
24	.65	2-group	Thought of an engine. Beats said One', two. Found that I was beating time with my thumb, and thought how funny I must look. Then lost count.

This *O*, unlike the former, is rhythmically-minded. It was found possible, by indirect suggestion, to induce 3-, 6-, 8- and — though with much greater difficulty — 5-groups.

EXPERIMENT (3). — This is Bolton's experiment. Its object is twofold: to establish the normal rhythm of the various *O*'s, and to estimate the relative difficulty of grouping by 2, 3, 4, 5, etc.

The method is that of indirect suggestion. If the "subjects had spoken of the clicks seeming like the clock ticks, they were asked if there was the same difference of intensity or quality in the sounds as was apparent in the clock ticks. . . . If they had said the sounds were all alike, they were asked why they had said sounds and not sound. Did they suppose there was more than one sound? . . . In some cases it was sufficient to ask the subjects to count the clicks as they heard them, and then to ask how they counted. . . . Again . . . a subject was asked why he tapped every fourth or second click, and so his attention was called to a grouping that was going on unconsciously" (185). Questions of this sort will bring out the rhythmical preferences of *O*'s who are at all rhythmically-minded.

It is needless, in view of Bolton's work, to cite results in full. The following is the first series with suggestion taken from an *O* whose natural rhythm is the 4-group

Exp.	Interval	Grouping	Remarks
1	1.2	4 in 2-groups	Accent on first and third. Second beat blacker than rest.
2	.45	4-group	Beats less heavy: visualised walking.
3	.31	8 in 4-groups	Inhaled on first and fifth; exhaled on third and seventh.
4	1.4	4 in 2-groups	As 1. Breathed in time to ticks. Moved head and right leg.
5	.65	8 in 2-groups	Accent on second beat of each 2-group. Visualised numbers.
6	.28	8 in 4-groups	Accent on first and third of each group. Saw dust following a running horse. First group slanted to right, second to left.
7	.39	4-group	Hydraulic pump keeping time to metronome. Each group distinctly separated from next following.
8	.39	5-group	Group came naturally, and yet was difficult to hold. Principal accent on first.

Exp.	Interval	Grouping	Remarks
9	1.4	4-group	Very disagreeable; feeling of effort. Hammer striking anvil.
10	.39	4-group, then 8 in 4-groups, then 8-group	Pleasant rate.
11	.39	2-group	Tried to make 3-group and could not. Effort made head ache.
12	1.2	4-group	Head moved. Group had form of rhomboid.
13	.3	4-group	Main accent on first beat, with deep inhalation. Suggested goblins playing on mound. Thought of music and dancing.
14	.9	3-group	Third beat accented; seemed to turn back, so as to lie between first and second. Saw blocks with red stripes.
15	.65	No group, then 5-group	Disagreeable; no group could be made. On direct suggestion, 5-group formed with effort. Still disagreeable.
16	1.2	4-group, then 3-group	4-group came naturally. On direct suggestion, changed (with feeling of jump) to 3-group, with accent on first.
17	.3	4 in 2-groups, then 3-group	Disagreeable; jockey riding in circus-ring. On direct suggestion, 3-group made with difficulty and discomfort.
18	.39	4-group, then 5-group, then 3-group	Sound unpleasant; pleasant when grouping began. 5-group suggested, and made easily. 3-group suggested; made by counting.
19	.65	4-group, then 3-group	Visualised green. 3-group suggested; made easily. Sounds seemed uneven; formed figures, polygons.
20	.9	4-group, then 3-group	Visualised glass tubes. 3-group made at suggestion. Accent irregular, and 3-rhythm unpleasant.

These three sets of Results were obtained from *O*'s who were either entirely unpractised or (as in the last case) had had no more training in the introspection of rhythm than the preliminary experiments afford. With practice, the rhythmical attitude of *O* becomes steadier, and there are fewer irrelevancies in the introspections. Nevertheless, the elements of the perception of rhythm can be teased out, without difficulty, from the Remarks. "An exhaustive description of the rhythmical consciousness,"

writes Meumann, "would have to distinguish, in any case, elements of time, elements of accentuation, intellectual processes of an associative and apperceptive character, emotional facts [direct effects of sense-feeling, and æsthetic feelings: 265], organic and motor concomitant phenomena" (280). All these part processes can be traced in the records.

Question (1). — This Question has already been answered. Exps. (1) and (2) bring out the fact of subjective accentuation, a grouping "accomplished by accenting regularly certain sounds more than others," with intervals between the groups "which are apparently longer than the interval which separates the individual clicks" (Bolton). Exp. (3) brings out the facts that normal grouping is, in the majority of cases, a grouping by 2 or 4 (Bolton, 212); that 3-groups and 2 × 3-groups can be formed fairly easily, on suggestion; and that 5-groups are difficult, whether to form or to maintain.

When the fact of subjective accentuation has been remarked, the student may be asked to mention analogies from other sense-departments. Vision furnishes some striking examples. Rule a series of vertical black lines, some 5 mm. apart, on white paper. Not only does the eye 'take them in' in groups of 3 or 4, but the white interspace between group and group seems to be broader than the space between line and line. Divide up a square, 10 × 10 cm., by black lines, drawn chess-board fashion, 5 mm. apart. It is possible to single out larger and smaller squares, or other figures, from the uniform pattern. If this is done, the boundary lines of the selected figure seem to be darker than the other lines of the chess-board. See J. Henle, Anthropol. Vorträge, ii., 1880, 47; F. Schumann, Zeits. f. Psych., xxiii., 1900, 7, 11.

Does subjective accentuation imply an intensification of sensation? Meumann leaves the question open: 302. Stumpf, admitting that (and explaining how) very weak sensations are actually intensified by attention, inclines to a negative view. Tonpsychologie, i., 373 ff.

Subjective accentuation has been observed by many authors. Besides Henle and Stumpf, we may mention G. Dietze, Philos. Studien, ii., 1885, 369; G. Martius, Philos. Studien, vi., 1891, 196 f.; J. Angell and A. H. Pierce, Amer. Journ. of Psych., iv., 1892, 534, 539.

It is to be noticed that *O*'s preference for a given form of rhythm, in exp. (3), is in large measure independent of the rate of succession of the clicks. This is important, in view of the further fact that the most pleasant or 'natural' period for the rhythmical unit, no matter what number of terms it may comprise, is practically constant at 1 sec. Require *O* to tap with a pencil on the table (or better with a single-click telegraph key) the 2-, 3- and 4-group rhythm that

is most 'satisfying' to him, that he feels to be 'right.' The rate of tapping quickens with increase of the number of terms in the rhythmical unit, so that the time occupied, say, by ten units, is approximately the same in every case. 5-, 6- and 8-groups may be tried; but the experiment is then complicated by the difficulty of tapping with the needed rapidity. — Meumann, 317, 427; for an exact method of determination, see Bolton, 214, 216.

(2) Meumann (303) answers this question as follows. (i) We find an apparent alternation in the accent or weight of the impressions. (ii) The periodic alternation of accented and unaccented impressions is heard as an alternation of intensities. (iii) As a rule, the rhythmical group begins with the term on which the principal accent falls. The groups are clearly separated. (iv) This separation may take on a temporal form: the terms of the group come in quick succession, while there is a pause between group and group. — *E* should also be on the watch for (v) reports of the apparent lengthening of the accented clicks, and (vi) apparent differences of time interval within the more complicated rhythmical units.

Experiment (4). — There are many ways of varying the intensity of the metronome clicks. A window, closed by a spring, may be let into the face of the box; the instrument may beat before the opening of a tube, at the far end of which *O* sits, and the sounds be interrupted by a falling screen; a stop-cock may be introduced in the length of the tube, etc. The method of the text is the simplest, and (in the author's experience) entirely satisfactory.

For Results, see Bolton, 226. The Instructor must use his discretion in the choice of *O*'s for this and the three following experiments.

Question (3). — "The group must either begin with a very intense sound or close with a very weak one. The subject strives either to put all the strong sounds as near the beginning as possible, or all the weak ones as near the close as possible. . . . The general principle is well illustrated in the last two forms of the 5-groups" (Bolton).

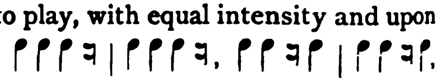
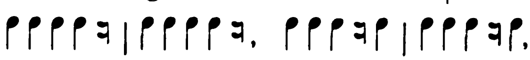
(4) The following points may be noticed. (i) Under certain conditions, *O* hears not two but three degrees of intensity. Thus the rhythm $1'\ 1'\ 1$ is heard as $1'\ 1''\ 1$; and $1'\ 1'\ 1'\ 1$ as $1'\ 1'\ 1''\ 1$.

Bolton explains the phenomenon by contrast (227). (ii) The experiment brings out very clearly the intimate connection (which is, indeed, an interchangeability, or vicarious operation) of the part-factors in rhythm. To most *O*'s, the stronger (or strongest) sound appears longer than the rest: intensity = duration. The strongest sound "spreads itself over" the rest. Again, the introduction of the stronger sound effects a change of subjective time-interval; in general — the complication of factors is too great to allow us to lay down an exceptionless rule — the interval following the intensive sound is lengthened. Here, then, intensity = temporal disjunction. Bolton, 228; Meumann, 305, 311. These results pave the way for the three following experiments, in which we are to make duration, temporal disjunction and quality = intensity.

EXPERIMENT (5). — It is well that *O* should be kept in ignorance of the order of the three following experiments. Hence the lack of details in the text.

E is to play, with equal intensity and upon the same digital, the series  etc. The rhythmical effect is unmistakable. The longer sound appears more intensive than the rest, and the familiar illusions of interval are aroused. Meumann, 61.

If *E* distrust his ability to strike the key uniformly, the Instructor may play for him. It is not difficult (but it is also not necessary) to devise a system of levers for striking the key with equal force. The time of pressure may then be regulated by a 'soundless metronome,' — a useful laboratory appliance, consisting simply of a string and bob, whose rate of vibration has been determined by aid of a stop-watch.

EXPERIMENT (6). — *E* is to play, with equal intensity and upon the same digital, the series 
 etc. The rhythmical effect is, again, quite clear. In most cases, the sound that is followed by the rest appears louder than the others, and also seems to begin the rhythmical unit. There are, however, individual differences in the manner of grouping.

The experiment may also be performed with the boxed metronome. *O* sits at such a distance from the box that closure of the lid entirely destroys the sound of the click. *E* starts the metronome at one of the more rapid rates, and cuts out every fourth, fifth, etc., beat by dropping the lid. The results in this case are modified by the length and invariability of the pause.

EXPERIMENT (7). — *E* plays, in regular alternation and with equal intensity, the notes c^1–e^1. The change of quality will mean, for most *O*'s, a change of accent, *i.e.*, a rhythm. *E* may then go on to play c^1-d^1-e^1; then c^1–d^1–e^1–f^1, etc., etc.

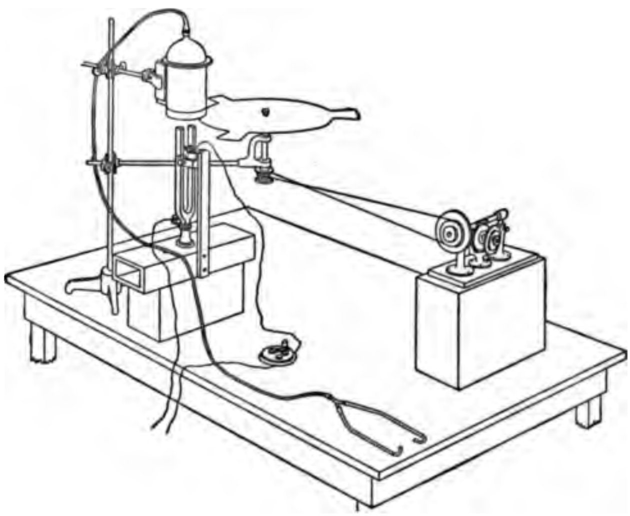


FIG. 99. — Rhythm apparatus. The belt runs to a Pillsbury speed reducer, from which it is carried to the motor (not shown in the Fig.). — The speed reducer is sold by the Michigan App. Co. for $12.

The author must confess his failure, after many attempts, to devise a cheap apparatus that shall satisfy the requirements of Exps. 5–7. Sanford has suggested an arrangement for varying the duration and intensity of tonal stimuli, which is figured by Bolton, 229. It is an adaptation of a beat instrument devised by A. M. Mayer (Amer. Journ. of Science, Ser. 3, viii., 1874, 241; xlvii., 1894, 5), and consists of an electrically driven tuning-fork (say, of 250 vs.), vibrating over the mouth of the appropriate Helmholtz or Kœnig resonator. Within the space (not more than 1 cm.) between fork and resonator travels the margin of a heavy junk-board disc, 50 cm. in diameter. The disc revolves once in the 1 sec. A rubber tube, ending in a two-way ear-tube (phonograph tube), may be led off from the farther end of the resonator.

It is clear that, if the margin of the disc is continuous, *O* will hear no sound from the fork. If portions of the margin are cut away, to a sufficient depth to expose the prongs of the fork, there will be an alternation of sounds and silences. With cuts of an equal number of degrees, equally spaced, we shall get a series of sounds alike in pitch, duration, intensity and time-interval. By varying the length of cut and of interspace, we can vary the duration and time-interval of the tones. Further, by cutting the margin only to such a depth as will expose a single prong of the fork, we obtain a sound of greater intensity than the rest. Hence, if the revolution of the disc is constant, we have the three variables intensity, duration and interval under good control.

In Sanford's form of the apparatus, the disc stands vertically and is turned by hand. Owing to the weight of the junk-board, and the extreme irregularity of form of certain discs, it is better to let the disc rotate in the horizontal plane. And it is (in the author's experience) essential that the turning be done by some constant source of power (*e.g.*, the motor of the Edison phonograph, Class M). The diagram represents this modified arrangement: the belt runs from the disc-support to a Pillsbury speed reducer (transmitter), by which it is connected with the motor.

The following discs are recommended by Bolton, and will be found useful for preliminary tests.

(1) 2 notches of 150° each; one accented. Pauses 30°. Measure: ∠ — or — ∠.

(2) 1 notch of 200°, one of 100°. Pauses 30°. Measure: — ◡ or ◡ —.

(3) As (2), except that the 200° notch is accented. Measure: ∠ ◡ or ◡ ∠.

(4) 3 notches of 100°. Pauses 20°.

(5) 3 notches of 80°. Pauses 40°.

(6) 3 notches of 100°, one accented. Pauses 20°. Measure: ∠ — —, — ∠ — or — — ∠.

(7) 1 notch 120°; 2 notches 60°. Pauses 40°. Measure: — ◡ ◡, ◡ — ◡ or ◡ ◡ —.

(8) As (7), except that the 120° notch is accented. Measure: ∠ ◡ ◡, ◡ ∠ ◡ or ◡ ◡ ∠.

(9) 4 notches of 60°; one accented. Pauses 30°. Measure: ◡́ ◡ ◡ ◡, etc.

The discs may be taken in order, (1) to (9). *O* is to listen, in each instance, until such time as a definite subjective rhythm has taken shape: the period varies from 20 to 80 sec. The following are the rhythms preferred by two *O*'s: the results are averaged from 7 series of tests.

Disc (1) ∠ ∪; ∠ ∪. **Bolton**: ∠ —.
" (2) ∪ ∠; ∪ —. **Bolton**: ∪ — or ∪ ∠.
" (3) ∪ ∠; ∪ —. **Bolton**: ∪ ∠.
" (4) 10-group; ∠ ∪ ∪. **Bolton**: 4-group or 3-group.
" (5) 10-group; ∠ ∪ ∪. **Bolton**: ∠ — —
" (6) ∪ ∪ ∠ or ∠ ∪ ∪; ∠ ∪ ∪.
" (7) ∪ ∪ ∠; ∪ ∪ ∠. **Bolton**: ∪ ∪ —.
" (8) ∪ ∪ ∠; ∪ ∪ ∠. **Bolton**: ∪ ∪ ∠.
" (9) ∠∠ ∪ ∪́ ∪; ∪́ ∪ ∪́ ∪.

We notice here (*a*) the fact of subjective accentuation: discs (4) and (5). The 10-group was formed by counting (at first unnoticed by *O*); it consisted of 5 2-groups. (*b*) The accented term imposes a rhythm by twos, threes, fours, etc. (*c*) The longer sound imposes a similar rhythm. Other results are: (*d*) a temporal displacement, due to the accented or to the longer term; (*e*) a subjective increase in the duration of the accented term; (*f*) illusions of pitch, the louder sound appearing the higher; (*g*) the introduction of weaker secondary accents in the 4-groups.

This apparatus, given the power supply, is easily adjusted, and the discs are capable of wide variation.

Question (5). — The experiments demonstrate that the various part-conditions of rhythm may function vicariously one for another; that they are, in reality, coördinate and independent. We have as yet no psychological theory of this interchangeability. See Meumann, Philos. Studien, ix., 1894, 305 f.; x., 1894, 305.

Related Experiments. — We may mention here certain temporal illusions which stand in a close relation to rhythmisation.

(1) Let the metronome beat, at a given rate, first for two or three clicks only, and then for a longer time. Note that the series seems to run much more quickly than the separate ticks.

Try the experiment with various rates of beating.

(2) Let the metronome beat, first slowly and then quickly, for 20 ticks. Note that the quicker series appears the louder.

Repeat the experiment with the reverse order of stimulation, and with different rates of beating.

(3) Place the metronome in its box, and close the lid. When the ticks have sounded for some little time, raise the lid for a single beat; then close it, and let the muffled ticks continue. Note the shift of time-interval produced by the more intensive beat.

Try the experiment with various rates of beating.

(4) Place the metronome in its box, and close the lid. When 20 ticks have sounded, raise the lid for another 20 ticks. Note that the louder series appears the quicker.

Repeat the experiment with the reverse order of stimulation, and with different rates of beating.

See Meumann, Philos. Studien, ix., 1894, 274 ff.; x., 1894, 311.

To these may be added the following experiment, which confirms the statement that there is a natural tendency to place the accented beat at the beginning of the rhythmical unit (*cf.* Question 3 above).

(5) Place the metronome in its box, and sound the rhythm 1–2–3′ or 1–2′–3. Note that, in a little while, the former rhythm changes subjectively to 3′–1–2, and the latter to 2′–3–1.

Require *O* to tap, on a single-click telegraph key, the rhythm 1–2–3′. Note that, if the tapping is continued long enough, the rhythm invariably changes to 3′–1–2.

See Meumann, Philos. Studien, x., 286; Bolton, 222, 231.— For other experiments, see K. Ebhardt, Zeits. f. Psych., xviii., 1898, 99.

Question (6). — If the question is not understood by the students, it may be put in this way: Is it right to call rhythm a perception? Might it be classified elsewhere in a psychological system? What are the grounds on which you make your choice?

According to Meumann, "the specifically rhythmical consists essentially in intellectual acts, to which occasion is given (under certain conditions) by a rapid succession of determinate sensations." That the intellectual processes "are the *prius* in the total rhythmical perception" is shown by such facts as the following.

(1) Subjective rhythmisation is always introduced by apparent periodic alternations of intensity, which effect a subordination of weaker to stronger and a coördination of the stronger impressions; and by an *innerliches Zu-*

sammenfassen of weaker and stronger, etc. (2) These intellectual processes are independent of affective change, and are compatible with a state of indifference. (3) The greatest energy of 'internal grouping' runs parallel with a very slight feeling-effect (slow rhythms). (4) The measure in subjective rhythmisation may be altered by simple ideation of a different measure. — Philos. Studien, x., 272 f., 284.

It then remains to explain the motor phenomena, feeling effects and organic changes which accompany the perception of rhythm.

M. K. Smith (Philos. Studien, xvi., 1900, 291 f.), at the conclusion of a later investigation carried out in Meumann's laboratory, writes as follows.

"Rhythm is, according to Wundt, a progressive emotion (*Affectverlauf*), in which there is a regular alternation of expectation and satisfaction. The author is inclined, from her experience, to say that Wundt has here given, in a few words, the best explanation of the psychological nature (*Wesen*) of rhythm that we yet have. . . . Rhythm may be designated an emotion, whose motor (and, in part, whose vasomotor) expressions and discharges cannot take place with entire freedom, as in the ordinary progressive emotion, but whose expressive movements are regulated, temporally and intensively, by a determinate schema. Rhythm is (so Meumann modifies Wundt's view) an emotion, which discharges itself in ordered movements." This order extends to the movement pauses, to the time of initiation of movement, to the time occupied by the movement, to the gradation of intensities of impulse, to the course of acts (pulses) of attention and probably to vasomotor processes. "The psychological condition of rhythm at large is the rapid resolution of expectation."

It is evident that there is, here, a change of view on Meumann's part. The matter is interesting, and systematically important.[1] In order to make a decision, the student will be obliged to form a very clear and precise idea of what is meant by 'perception' and 'emotion.' If he becomes impressed by the fluidity of the processes covered by these and similar functional terms, so much the better.

As the author has worked out the rhythm experiment, emphasis is laid rather on the perceptive than on the emotive constituents of the rhythmical

[1] Stumpf remarks (Tonpsychologie, i., 135): "Judgments of time and of intensity are connected in the apprehension of rhythm: but we shall best treat of that in the course of our doctrine of feeling."

consciousness. The Instructor should point out this fact, and draw the students' attention to the affective elements in the introspective records. *Cf.* the answer to the following Question.

Question (7). — In the foregoing experiments, we have varied the sense-material of rhythm: we have had an uniform sound series, followed by series with intensive, temporal (duration and interval) and qualitative changes. It remains only, under this head, to vary the filling of the intervals; to compare the rhythm of 'empty' intervals with that of intervals, marked off by sounds, and filled with other sounds, with sights, etc.

We have then to investigate the organic changes that accompany the rhythmical perception. Breathing deserves especial attention. Meumann has proved that respiration adapts itself to rhythmisation: a change in subjective accentuation is followed by a change in breathing (270, 272). *Cf.* Mentz, Leumann, Dogiel, Dutczinsky, as cited by Meumann; Bolton, 202; and references in the introspective reports quoted above.

We have, thirdly, to study the associative and interpretative ideas that accompany the rhythmical perception: Meumann, 265; Bolton, 184 ff.

Fourthly, we must examine the affective side of the rhythm consciousness. We must note, and seek to analyse, (1) the feelings interwoven with the sense-material as such: feelings of stimulation, of expectancy, of confusion, of painful slowness, of simple agreeableness or disagreeableness, of unrest and uneasiness, of effort and discomfort, of annoyance, of drowsiness; (2) the emotive forms accompanying the perception: satisfaction, pleasant animation, excitement, gravity or 'staidness,' melancholy, cheerfulness, dread, torment; and (3) the æsthetic sentiments, of completeness, rightness, 'being rounded-up,' 'restful evenness,' intrusion, ease. — Meumann, 264 ff.; Bolton's introspections, 186 ff.; *cf.* the series quoted above.

Finally, we must observe and classify the motor expressions of the rhythm-feeling, or the motor concomitants of the rhythmical perception. It may be noted here that, according to Smith, "there is a constraint towards motor rhythmisation of continued movements, just as there is towards the subjective rhythmisation of sound impressions" (*loc. cit.*, 282). This remark leads us to the

general question of tactual rhythm, which requires a programme of its own.

Question (8). — Meumann (306 ff.) gives 5 points of difference. (1) Besides grouping in terms of time and intensity, we have a grouping dependent on the inner connection of the tones, *i.e.*, phrasing. (2) The tones within the *motif* have varying values. Some dominate, expressing the culmination of the musical thought; some furnish a preparation for this thought; in others it works itself out. (3) The tonal variation gives greater space and freedom to subjective rhythmisation. (4) Tones may vary in duration, as simple sound impressions cannot: *cf.* the hold. (5) The higher intellectual processes are enhanced.

Question (9). —The separate objects are successively apprehended: each in turn affords a fixation-point for the eye and a point of rest for the attention. The rhythm is tactual or 'motor,' set up by the alternation of rest and movement; and the muscles involved are those which subserve eye-movement and those which are normally concerned in the 'expression' of visual attention. — Meumann, 261 f.; Smith, 300.

This answer is obvious. The Question is introduced in order that the Instructor may, if he wishes, have a point upon which to hang a discussion of the relation of attention to rhythmisation. Wundt (the first to propound a psychological theory of rhythm), Meumann, Bolton and Smith have all a good deal to say upon this topic.

Question (10). — See Wundt, Phys. Psych., ii., 84 ff.; Meumann, 285 f.

Question (11). — References (and criticism) in Meumann, 252 ff. Stumpf (Tonpsychologie, i., 340) remarks that "our sense of time and rhythm appears to have developed, for the most part, in walking," and quotes Wundt (Phys. Psych., 2d ed., 1880, ii., 286) to the effect that the time-period which is most accurately reproducible in idea is practically identical with the time required for a movement of the leg in rapid walking. Wundt, however, gives up the fact, and the theory based upon it, in his later editions (*cf.* ii., 1880, 287 f.; ii., 1887, 354; ii., 1893, 416), though he still regards bodily movement as the ultimate source of the rhythmical impression (ii., 1893, 91; *cf.* 84). *Cf.* James, Psych., i., 560, 634.

LITERATURE. — W. Wundt, Phys. Psych., ii., 1893, 83 ff., 289; Vorlesungen über Menschen- und Thierseele, 1897, 433 (trs. 376 f.); E. Meumann, Philos. Studien, x., 1894, 249, 393; T. L. Bolton, Amer. Journ. of Psych., vi., 1893, 145, 310; M. K. Smith, Philos. Studien, xvi., 1900, 71, 197; M. Ettlinger, Zeits. f. Psych., xxii., 1900, 161 (a paper to be read in connection with Lipps' theory of the geometrical optical illusions).

EXPERIMENT XXXII

§ 53. **The Localisation of Sounds.** — Experiments upon localisation, in all sense-departments, tend to take on a purely quantitative form. The records then consist of tables of figures, showing the accuracy with which the position of the stimulus has been 'judged' or 'estimated,' without any attempted analysis of the process or mechanism of judgment itself. Sometimes there is appended to the report a 'theory' of localisation at large, a general statement of the physiological conditions under which a judgment of distance and direction is possible.

This tendency towards the substitution of columns of figures for analytic work must not, however, be too severely blamed. On the one hand, it is characteristic of a young science. The goal of science is quantitative formulation; and it is as inevitable that the pioneers of a new science exaggerate the exactness and finality of their results as it is that the trend of enquiry shall presently follow the opposite direction, of a precise qualitative analysis. The earlier workers upon localisation, the time sense, the various forms of reaction, the fluctuations of attention, etc., etc., set out to get figures and formulæ; and, when they had got them, naturally regarded their task as completed. On the other hand, this 'scientific' attitude to psychophysical problems was encouraged, so to speak, by the problems themselves. The task of introspection, in the cases cited and in many others like them, is immensely difficult: we have already said something of the difficulty in § 45. But, if there is no obvious material upon which introspection is to work, it is, again, natural that introspection shall be neglected. There is no lack of zeal or patience on the part of the investigator: it is simply that, at the stage

of the science which we are now considering, the problem does not present itself as an introspective problem.

Let us take the instance of localisation of sound. If we look at the problem abstractly, there seem to be three possibilities of solution. Our apprehension of the locality of sounds may be immediate: there may be a special organ, or specific coördination of organs, for judgments of up and down, left and right, before and behind. In this case, all that we have to do is to measure the accuracy of localisation in the different dimensions of objective space, and then search the physical organism for our organ. Preyer and Münsterberg find such an organ in the semicircular canals of the internal ear. According to Preyer, the ampullæ of the canals give us an immediate perception of the direction of sound; according to Münsterberg, we derive this perception, with equal immediacy, from the 'muscle' sensations attending movements of the head, which are reflexly released by stimulation of the ampullar apparatus. Secondly, however, our apprehension of the locality of the source of sound may be mediate or indirect. And the criteria by which we make our judgment of locality may, again, be either homogeneous or heterogeneous. The judgment may depend, *e.g.*, upon the relative intensity of the sound as heard by the two ears; a sound which is very loud to the right ear and comparatively weak to the left must lie towards the right of the head and body. Here we are perceiving direction of sound in terms of intensity of sound: the criterion is homogeneous. But the judgment may also depend upon tactual perception. "Since the various parts of the shell of the ear possess a delicate sensitivity to pressure, a sensitivity which is enhanced anteriorly by fine hairs, the tactual sensations of the two pinnæ must (especially in the case of intensive sound impressions) be differently distributed according to the direction of sound" (Wundt). It is also possible that specific sensations are set up by the movement of the tympanic membrane, and by the contraction of the tensor tympani. Or we may have recourse to criteria of a still more remote kind. The spatial position of the source of sound may be visualised, as soon as the sound is heard; or a reflex movement of the eyeballs (or impulse to such movement) towards the source of sound

may follow the auditory sensation. We should then be in presence of one of those mental short-cuts of which we have spoken above (p. 129 of the text). In all these instances, the criterion is heterogeneous.

There seems to be no doubt, in the present status of the problem, that localisation is mediate, and not immediate. The localisation experiment (here as in the case of touch) thus becomes a qualitative experiment. For the remoter secondary criteria we may appeal to introspection, with good prospect of a successful analysis. Many judgments of direction and (so far as we can tell) all judgments of distance are referable to 'association,' and the unravelling of the associations is not a very difficult task. On the other hand, introspection finds it as hard to deal with the homogeneous and the more direct heterogeneous criteria as it does to deal with the local signs of touch and sight. We must, therefore, try to assist it in every possible way by variation of our experiments. At the best, however, the introspective harvest will be scanty. The value of the experiment lies, then, quite as much in what may be called the 'qualitative attitude' of the student as in the positive introspections obtained.

It may be taken as a general rule of work in this and similar experiments that the quantitative procedure is incompatible with the fullest and most reliable introspection. When *O* is required to localise the source of sound, he naturally gives his complete attention to that problem. If he is asked, subsequently, to describe his method of localisation, to inventory the contents of the localising consciousness, he finds that method and material have, in large measure, escaped his notice, and that what he noticed has, in large measure, been forgotten. It is therefore essential that the mixed (quantitative and qualitative) series be supplemented by purely qualitative experiments, in which *O* is asked, not *where* he localises the sound impression, but *how* he would localise it if he were called upon to do so.

MATERIALS. — There are several forms of the sound cage. All are somewhat bulky, and the cheapest can hardly be made for less than $15. It may, therefore, be worth while to indicate a method by which the equator of the sound-sphere may be accurately explored with very simple apparatus.

E draws upon the floor a chalk circle of 1 m. diameter. The centre is clearly marked, and the circumference divided into 5° units. *O*'s chair is to be placed as nearly as may be at the centre of the circle; *i.e.*, in such a position that, when *O* is comfortably seated, a vertical dropped from the centre of the line joining his two ears would pass through the centre mark upon the floor. *E* must either remove his shoes or wear over-shoes, in order that he may move noiselessly. He needs a rod, padded at the lower end, and cut to such a length that when standing vertically upon the floor its upper end is exactly on a level with *O*'s ear, and a toy snapper.

The course of the experiment is then very simple. *E* adjusts the rod and snapper at some point upon the circumference of the circle. When *O* says "Ready!" the snapper is sounded: the signal must come from *O*, in order that *E*'s position may not be shown by his voice. *O* localises, whether by pointing or verbally (so and so many degrees), and *E* records the direction and amount of error. The procedure is adapted both for quantitative and for qualitative work.

For another form of sound cage, see M. Matsumoto, Yale Studies, v., 1897, 2. The limitation of the movement of the receiver by *O*'s body could be avoided only by making the cage large enough to contain a seated *O* within it. On the other hand, the interference of the iron standard at a critical part of the sound sphere (back-front confusions are common, and deserve special study) is a serious defect in the construction of the instrument. The author would advise, either that the cage be suspended from the ceiling, and steadied by light, movable supports, or that it be held from the sides as in the Yale model, but that the supports be placed at a greater distance from the cage itself. Preyer used a sound helmet, a cap set with wires which pointed in various directions and at the extremities of which the sound stimuli could be given. The cage is preferable.

It is curious that there has not been more discussion, in the literature, of the question of localisation methods. The author's experiments upon the matter seem to justify the statements (*a*) that localisation by the cardboard semi-circles is slightly more accurate, upon the average, than localisation by the pointer or by verbal description, and (*b*) that *O*'s mannerisms in localisation will, if carefully studied, throw some light upon the mechanism of the localising consciousness.

PRELIMINARIES. — The head-clip may need to be filed or padded, if *O*'s head is to be in the right position. Neglect of

this precaution has, in the author's experience, brought the ear 40 mm. below the receiver in the positions 25–25 and 25–75.

The semicircles can be set with sufficient accuracy to the half of a division. Two series of ten settings, with a position of the horizontal semicircle in which a difference of half a division on the scale corresponded to a difference of 10 mm. in height above the floor, gave mean variations of less than 0.9 mm. and less than 1.0 mm. respectively. It is not possible to set accurately at thirds or quarters of a division.

Some *O*'s are annoyed by the touch signal. It is then advisable that *E* should give the customary "Now!" pressing the key down as he utters the word. He must, of course, always stand in precisely the same place. The objection to this method is that *O* forms a very accurate idea of *E*'s position, and so has a constant direction with which to compare the apparent direction of the sound.

Note that, if the rapidity of the break be not constant throughout, the intensity of the click will vary. As any such variation is undesirable, *E* should be cautioned to withdraw his finger from the key in exactly the same manner in every test.

EXPERIMENT (1).—The experiment with partial knowledge is recommended as an introductory experiment, partly because it quickens the course of practice, and partly because it facilitates introspection. The key should be closed before the ready signal is given, for the reason that there may be a faint click at make. This passes unnoticed if *O* is not attending; it may serve to distract him if it follow the signal.

There will be several cases in which *O* is unable to give a definite localisation. It must then be left to *E*'s discretion whether he enter the *?* in the record, or repeat the test. If *O* is tired or inattentive, repetition is useless; if his inability to form a decision is due to hesitancy between alternative directions, it may be worth while to repeat the click as many as half-a-dozen times, in order that final judgment may be passed, and (what is more important) that the introspective reasons for such judgment may be noted.

The numerical and introspective Results may be tabulated as follows.

No. of Exp.	Set:		Observed:		Remarks.
	V.	H.	V.	H.	
1	15	0	9	0	[Enter introspections here.]
2	25	10	18	10	
3	15	60	14	66	
4	35	20	38	11	
etc., etc.					

The mean error and its mean variation are then to be calculated, (*a*) in the vertical plane for each of the five settings 0, 5, 15, 25, 35; and (*b*) in the horizontal plane for each of the six settings 0, 10–20, 30–40, 50, 60–70, 80–90. Thus the *m. e.* and *m. v.* of V. 15 in 10 trials might be:

Errors	Variations
− 6	6.2
− 1	1.2
+ 5	4.8
− 3	3.2
+ 3	2.8
+ 4	3.8
− 1	1.2
− 2	2.2
+ 3	2.8
0	0.2
m. e. = + 0.2	*m. v.* = 2 84

The introspective reports will hardly contain more than the following.[1]

(*a*) Certain of side, and comparatively sure of direction; less sure of height. Localisation as quick as perception of the sound.

(*b*) Back, left; both certain. No visualisation.

(*c*) Opposite right ear. Easy.

(*d*) Vertex: doubtful. Had a strong bias for low and back.

(*e*) Doubtful as to back or front: had to choose back. Curious feeling of uncertainty as judgment alternated.

(*f*) Sure of median. Distinct lifting eye-movement.

(*g*) Judgments rougher when click is as high up as this: I don't like to strain my eyes up so far.

[1] These reports do not correspond to the experiments of the Table quoted above.

That is to say: *O* is able to describe the judgments as mediate or immediate, to indicate some of the more remote secondary criteria, to give the degree of certainty with which a judgment is passed, and to note the influence of expectation, habituation, etc.

The three series of experiments will have brought out *O*'s preference for a particular method of localisation (visual, tactual, verbal). This method should be adopted for the following experiments.

EXPERIMENT (2).—The fifty tests should be distributed symmetrically over the available surface of the sound sphere. To save time in setting the receiver, the numbers V. 0, 10, 20, 30, 40, and H. 0, 10, 20, 30, 40, 50, 60, 70, 80, 90 may be taken as the basis of the Table. The remaining tests should be taken from the high and low regions on either side of the median plane, back and front. Errors and variations should be calculated as before. The introspections will be, perhaps, even more scanty than those of exp. (1).

The second list of fifty should be made out with special reference to constant tendencies or preferences on the part of *O*. Suppose, *e.g.*, that he shows a distinct leaning towards 15–80. There may be something in the disposition of the apparatus, or of the surrounding surfaces, to favour that point; or there may be a constant difference of intensive sensitivity between *O*'s two ears. The former source of error should have been guarded against from the outset: the room should have been carefully tested for echoes, and curtains hung where any such disturbance was found or even suspected. A test may now be made, by turning the sound cage, say, through 90°, and noticing whether the 15–80 tendency persists.[1] As for the possible difference between the two ears, most men hear more intensively with the left than with the right ear (*cf.* the 'better ear' of Exps. V., IX.; Fechner, Abh. d. kgl. sächs. Ges. d. Wiss., vii., 1860, 541; Stumpf, Tonpsychol., i., 1883, 364). A test may be made either directly, by noting the distance at which the ticking of a watch ceases to be audible for each ear (Sanford, Lab. Course, exp. 61), or in-

[1] Reflection of sound from the floor may be of influence, but cannot be eliminated. *Cf.* Matsumoto, Yale Studies, v., 1897, 7.

directly, by plugging the two ears successively in exp. (3) below, and noting the amount of lateral displacement upon the equator of the sound sphere in each case. If the preference is not explicable in terms of these two sources of error, — and if careful consideration fails to bring out any other suspicious circumstances in the conduct of the experiment, — it is presumably the expression of some mental trend or bias, or the outcome of some association, which thus invites enquiry. The series should then be made out with the view of determining (*a*) the limits within which the bias is effective, *i.e.*, the exact range of scale-divisions in the two planes which evokes the judgment "15–80"; (*b*) the effect upon the judgment of different modes of approach (by wide jumps, or by small steady steps) to the 15–80 region; (*c*) the presence or absence of preference for the symmetrical regions 15–20, 35–30, and 35–70; and so forth. The introspections should be carefully noted; especially should the degree of confidence with which the localisations are made be recorded: and the click may be repeated as often as *O* desires.

If no constant tendency has appeared, the second series may repeat the tests of the first, in a different order.

Results. — The general results of these series may, in all probability, be summarised as follows.

(1) There is no confusion of right and left.

(2) There is no confusion of right or left and median.

(3) There are confusions of above and below, before and behind.

(4) Localisation is most accurate in (or about) the transverse or auditory axis and in the horizontal plane.

(5) There is no constant difference in accuracy of localisation (*a*) between the upper and lower hemispheres, or (*b*) — unless *O*'s ears differ in sensitivity — between the right and left hemispheres. On the other hand, (*c*) localisation in the front is somewhat more accurate than that in the back hemisphere.

(6) An error, positive or negative, once established at a given point, is likely to be carried in the same sense through the whole number of settings on that point.

(7) Localisations given as 'doubtful' are as a rule largely in error; but localisations given as 'sure' are not always correct.

Individual differences, *e.g.*, as regards (3), should be noted by the Instructor. They may sometimes be accounted for, at least

conjecturally, by differences in the shape of the pinnæ, irrepressible tendencies to head-movement, etc.

EXPERIMENT (3).—The object of this experiment is to test the hypothesis that sound localisation depends, primarily at least, upon the relative intensity of the sound as heard by the two ears. If the hypothesis be correct, there must be a general shift of localisations: a sound given at 25–0, *e.g.*, will be localised, not in the median plane, but in the direction of the open ear.

The elimination of one ear is by no means an easy matter. It is possible, by closing or shading one eye, under certain general conditions of illumination, to secure a satisfactory monocular observation; and even if retinal rivalry supervene, there are moments when the field of the open eye is unaffected by that of the closed eye. In closing one ear, on the other hand, we are merely substituting internal for external stimulus. The stopped ear rustles and throbs and buzzes in what may be a very distracting way. All that we can do, therefore, is to make the internal stimulus as constant as possible, while we hold the attention as steadily as we can upon the external stimulus. A tightly fitting plug of cotton wool and wax answers the purpose fairly well. Cotton wool alone is not so good. Best of all, if it can be procured, is one of the conical eraser-caps sold for attachment to a lead-pencil. The cap has a cylindrical bore, which can be filled with wax to give the plug greater firmness; its conical form renders it adaptable to any ear.

The effectiveness of the plug may, if the Instructor think it worth while, be tested by the watch-tick experiment mentioned above, or by connection of the telephone receiver to an inductorium, as in the experiment on distance, p. 371 below.

The numerical results will be somewhat as follows:

No. of Exp.	Set:		Observed:	
	V.	H.	V.	H.
1	15	0	14	94
2	25	20	15	18
3	0	0	8	97
4	15	90	23	77
5	40	10	35	0

etc., etc. The mean errors and their mean variations are to be calculated as before. The introspections will still be scanty.[1]

(*a*) To right of median: wanted to make it median, but could not.

(*b*) Fairly sure, nearly opposite left ear: distinct eye-movement.

(*c*) Very doubtful of front or back.

(*d*) Doubtful between front and back: when click was repeated, was quite sure of back; immediate judgment.

(*e*) Opposite left ear: very clear and sure. — And so forth.

EXPERIMENT (4). — It is important that *O*, in entering upon this experiment, clearly represent to himself the intrinsically non-spatial character of sounds, or clearly call to mind the associative character of auditory space. He may say to himself something like this: "The sounds that I am going to hear are, in themselves, not localisable; if I localise them, it must be, in the last resort, by way of some association with touch or sight; even a difference of intensity in the two ears has to be interpreted, if it is to be put to spatial use, and the interpretation must be in tactual or visual terms; let me, then, be on my guard not to read into the sound impressions characters that really attach only to their tactual or visual associates." On the other hand, it is important that a successive association be not confused with a real criterion of locality. *O* may first localise, and *then* have a visual picture or a reflex movement of the eyes: the picture and movement (or movement-impulses) are, in such a case, not concerned in the mechanism of judgment. He should, therefore, go on to say: "What I have to analyse is the immediate datum of consciousness; I must say what I experience at the moment of perception of the sound; if there is nothing but a given 'thereness,' I must report that; if there is doubt, I must analyse that; if there is a medley of perceptual material from various sense-departments, I must put the whole situation into words; if an associated idea crops up after I have localised, I must report it as a later process;" and so on. Some *O*'s will make nothing at all of the test, and will be apt to pride themselves upon their failure; others will secure a few positive results, as well as a useful training.

[1] These reports do not correspond to the experiments of the Table quoted above.

The following introspections were taken with random settings of the semicircles. The settings were, unfortunately, not recorded in units of the instrument, and, in the majority of the tests of the first series, no attempt was made to determine whether the localisation was correct. *O* gave his full attention to the qualitative features of the introspection.

If the Instructor wish to combine both the qualitative and the quantitative features in the record, the following procedure should be employed. *O* formulates his qualitative introspection (including a verbal localisation) before he opens his eyes. Then, the cage remaining in its original setting, he opens his eyes, and the error of localisation is measured according to his directions. After this, he dictates his qualitative analysis from memory. The method 'works,' at least after a little practice; but there is probably some loss of accuracy on both sides.

Series 1

The report given under *Localisation* is not analytic, but represents a judgment made after the qualitative analysis had been performed.

1. Accommodation (centrally excited?) to visual source of sound. *Localisation:* front median.
2. Verbal-auditory 'right.' Eye-movement (centrally excited?).
3. As 2: but seemed to come *after* the localisation had been made.
4. Verbal-auditory 'left.' The sound was 'placed' in a vague visualisation of the room, cage, receiver, etc. Also c. e. eye-movement.
5. No analysis.
6. Verbal-auditory 'overhead.'
7. There was a preliminary click at the make. I consequently directed my attention to the right; I felt that I was anticipating 'rightness' both with ear and eyes.
8. Eye-movement (?) and vague muscular pulls on right side of body. Also the vague visualisation of 4. *Loc.:* right.
9. No analysis. *Loc.:* left back.
10. Known at once as up-front. No analysis possible.

Rest of 5 min.

11. Eye-movement, and the visual placing of 4 and 8.
12. Louder in left ear.
13. C. e. strain-sensations in eyes and arms, as if I were pointing to the receiver.
14. Period of doubt as to the altitude. A distinct idea of running my eyes up and down a vertical line. There was a 'feeling' for the proper place.

15. A very certain 'feel' of some sort about my eyes, as if I were directing my attention visually to the left.

16. No analysis. *Loc.:* vertex.

17. As 15, with up for left.

18. As 15. The eye 'feel' is followed directly by a verbal-auditory symbol, 'median front.'

19. No analysis. *Loc.:* opposite left ear.

20. Visual elements, mainly; but vague.

Rest of 5 min.

21. C. e. eye-movement sensations; vague visual placing, as in 4 and 8; verbal-auditory 'up there.'

22. Tried several times: 10 or 12 clicks. Very uncertain as to front or back. Knew it was level with ears and median. During the conflict, I could make it front or back by expectation. Distinct movements of scalp and eyebrows, and always a visual placing, while attending to front or back. *Loc.:* front. Correct.

23. Eye-movement and visualisation as before. *Loc.:* knew it was 'right,' but estimated much too high.

24. Very full sound in left ear. *Loc.:* opposite left ear. Correct.

25. Visualisation of the receiver, as in 4. Imaged arm-movement (pointing). Later: verbal-auditory 'back.' *Loc.:* low back. Correct.

26. As 24, but opposite right ear. *Loc.:* correct.

27. Visualisation and verbal-auditory 'vertex.' *Loc.:* correct.

28. In doubt between front, left, high (my place of preference in the previous series) and front, left, level. Distinctly visual elements during the conflict. — Setting was back, median, horizontal.

29. Full, intense sound in left ear. *Loc.:* opposite left ear. Too high; the receiver was below level.

30. C. e. arm-movement sensations (reaching to receiver) and eye-movements (looking at it). *Loc.:* front median. Correct.

Series 2

31. Eye-movement (c. e) upwards, and lifting of eyebrows. *Loc.:* front, left, high. Actual setting was back, left (near median) and high.

32. Reflex-like muscular movements in face. *Loc.:* left, back, horizontal. Correct.

33. C. e. eye-movement. *Loc.:* high, right, near median. Correct.

34. No analysis. *Loc.:* front, near vertex. Setting was near vertex, left, and back.

35. C. e. eye-movement and the vague placing outwardly in visual space. *Loc.:* too far front; setting was vertex, right.

36. Full intensity in right ear; also distinct tendency to eye-movement (still c. e., however). *Loc.:* opposite right ear, level. Correct.

37. No eye-movement; but distinct visual placing. Saw the receiver behind me. *Loc.:* directly behind, level. Correct.

38. Heard the make-click, and was sure it was opposite the right ear, level. But the break-click sounded higher. Tried 4 times to decide. Visualised in all trials. — It was really low, right, back.

39. Tried 3 times. Slight doubt between back low and front low (*i.e.*, between 62 and 87 on the horizontal circle). Vague 'external' visualisation and c. e. eye-movement. *Loc.:* front, left, low. Was back, left, low.

40. Distinct eye-movement: actually moved? *Loc.:* opposite left ear, level. Was really lower.

41. C. e. eye-movement and visualisation. *Loc.:* median, fairly high, front. Was slightly to left of median.

42. Analysis as 41. *Loc.:* opposite left ear, but low. Correct.

43. Analysis as in 41. *Loc.:* up, right, front. Correct.

44. C. e. eye-movement; less visualisation. *Loc.:* median, front. So far correct; but localised too low.

45. Vague picture of self in chair. Saw receiver as if from experimenter's position. Saw only the back of the chair distinctly. *Loc.:* low, median, back. Correct.

It is clear, from these records, that cases of confusion, *i.e.*, cases where a repetition of the stimulus is necessary to the formation of a judgment, are especially likely to throw light upon the mechanism of localisation. The change of attitude that constitutes the change from 'hereness' to 'thereness' gives a better opportunity for introspection than does the 'hereness' or the 'thereness' by itself.

Instructive results may be obtained from series where *O* is told beforehand, "You will be given a sound at this point or at that" (two possibilities), or, "You will be given a sound at so-and-so" (complete knowledge). Analysis must here be directed upon the processes involved in expectant attention.

Questions. — The first 5 questions have been sufficiently answered in what precedes. Question (6) must be answered, in part, from the literature. Question (7) should be approached methodically. The different part functions of space perception (the various space-determinations and space-relations) should be enumerated, and the three sense-spaces compared term for term. See Külpe, Outlines of Psychology, 334 ff., 374 ff.; J. von Kries, Zeits. f. Psych., i., 1890, 235 (an extremely suggestive

paper); A. Höfler, Psychologie, 1897, 342; A. Höfler and S. Witasek, Psych. Schulversuche, 1900, 24.

RELATED EXPERIMENTS.—There are many modifications of this Experiment, which serve to bring out the importance for localisation of absolute intensity, relative and absolute pitch, clang-tint, phase, the subjective factors of attention and fatigue, etc., etc. All of these variants must be taken into account in a final theory of sound localisation. We can here do no more than mention some of the most important experiments. It may be said that there is nothing in the results of any of them to cast doubt upon the hypothesis accepted in the text: that localisation depends primarily upon the relative intensity of the sound as heard by the two ears.

(*a*) *Variation of the Receiving Apparatus.*—Take a short series of tests, as in (2) above, with the pinnæ strapped flat against the side of the head. Take three further series, with the pinnæ strapped as before, but with two artificial pinnæ of cardboard tied to the head, their concavities pointing backwards, upwards and downwards.—J. Kessel, Arch. f. Ohrenheilk., xviii., 1882, 120.

Carefully fill the external ear passages with water, and plunge the whole head under water. Note that localisation (even as regards right and left) is impossible; sounds are localised simply 'within the head.'—E. Weber, Ber. d. kgl. sächs. Ges. d. Wiss., 1851, 30.

(*b*) *Variation of the Character of the Stimulus.*—Take a series of tests, before and behind in the median plane, with the tone of a tuning-fork, the clap of two wooden blocks, and a spoken word, as stimuli. Note the relative difficulty of localisation in the first case. The fork must, of course, be either so remote, or screened in such a way, that *O* does not hear the thud of the felt hammer as it strikes.—Rayleigh, Nature, xiv., 1876, 32; *cf.* Sanford, Course, 83, exp. 101 *d*.

(*c*) *Intracranial Localisation: Purkinje's and E. Weber's Experiments.*—(i) Connect the two ears by a piece of rubber tubing. Strike a tuning-fork sharply, and set its stem upon the middle point of the tubing. Note that the sound is heard in the occipital region of the head. Shift the position of the fork

upon the tubing; remove the tubing entirely from one ear: note the results. Repeat the experiment with a very weakly sounding fork. — K. L. Schaefer, Zeits. f. Psych., i., 1890, 300; M. Matsumoto, Yale Studies, v., 1897, 37; Sanford, Course, exp. 102. (ii) Strike a tuning-fork sharply, and set its stem upon the vertex. The tone sounds within the occiput, in the median plane. Now close the opening of one ear with the finger: the sound is heard within the closed ear. Repeat the experiment, with the fork placed at various points in the occipital and parietal regions. — Weber, *loc. cit.*, 29; *cf.* Sanford, Course, 84, exp. 103. Schaefer gives a pretty variant of the experiment. "Sing loudly a very deep *oo*, and while it is sounding stop one ear, not too closely. The *oo* shifts from the larynx to the closed ear. Now treat the other ear in the same way; the *oo* travels to the median plane within the head" (*loc. cit.*, 305).

(*d*) *The Effect upon Localisation of Fatigue and Attention.* — (i) Expose one ear for 30 sec. (by means of a tube connected with the resonator) to the tone of a c^2 fork. Now sound the same fork in the median plane (in the 'subjective' median plane, if *O*'s ears are unequal), and require *O* to localise it. He places it a little away from the median plane, in the direction of the unstimulated ear. An a^1 fork, or in general any fork of a different pitch, is unaffected by the c^2-fatigue. (ii) Sound two c^2 forks by striking the one upon the other. Hold them at equal distances from the two ears, or set their stems in the openings of the ear passages. You are able at will, according to the direction of attention, to localise the single resulting tone in the right or left ear. — Fechner, Abh. d. kgl. sächs. Ges. d. Wiss., vii., 1860, 549.

(*e*) *Localisation with Two Stimuli.* — (i) Hold two unison forks, sounding with equal loudness in opposite phase, close up to the two ears. Note that the resulting tone is localised within the occiput in the median plane. Try the effect of removing the two sources of sound, slowly and evenly, away from the ears. Repeat the experiment with sameness of phase. — S. P. Thompson, Phil. Mag., Ser. 5, 1877, iv., 274; 1878, vi., 383; Schaefer, *loc. cit.*; V. Urbantschitsch, Pflüger's Arch., xxiv., 1881, 579. (ii) Hold two unison forks, sounding at unequal intensities,

equidistant from the two ears. Note that the sound is heard exclusively by the ear on whose side is the louder fork. — Fechner, *loc. cit.*, 543 ff.; *cf.* Dove's experiment, 549 f. (iii) Systematic experiments may be carried out on the sound cage, with two telephone receivers. The results will be that, apart from occasional confusions of back with front and above with below, the resulting sound is localised by *O* at a point midway between the points at which the two stimuli are given. — Matsumoto, *loc. cit.*, 42 ff. On the conditions of separate localisation with simultaneous stimuli, see von Kries, *loc. cit.*

Schaefer recommends, for (i), two telephones connected to the two secondary coils of Preyer's double inductorium. The same arrangement will serve for (iii).

Matsumoto's apparatus is shown in the diagram. A fork of 250 vs. is placed as a shunt across the telephone circuit. The intensity of the tone in *a* is regulated by the position of the secondary coil; that of the tone in *b* by a copper-sulphate rheostat. The author has not tested this arrangement. An evident criticism is that, owing to the reciprocal effect of the secondary upon the primary coil, there will be intensification of the sound in *b* when the sound in *a* is intensified. As Matsumoto does not mention this source of error, it is probably negligible.

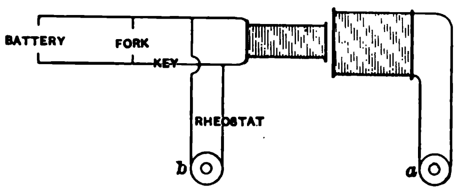


FIG. 100.

If the sound cage is not available, the present experiment may be performed by help of two cheap ('pipe metal,' open) organ-pipes, held upon upright stands, and connected by rubber tubing to a T-way and thence to a foot bellows. *Cf.* p. 359 above.

So far we have been dealing only with the apparent *direction* from which a sound comes. The problem of localisation includes, further, an enquiry into the apparent *distances* of sounds. All experiments go to prove that (as was said above, p. 358) our judgment of distance is a matter of association. A sound of known intensity is localised, by visual association, at a certain distance: similar sounds of greater intensity are then perceived as nearer, similar sounds of less intensity as farther off. The law may be roughly demonstrated by connecting the receiver upon the sound

cage with the secondary coil of an inductorium from which the vibrator and Helmholtz side-wire have been removed. Matsumoto thinks that, within certain limits, the "perceived distance of the sound increases in arithmetical progression when the intensity of the sound diminishes in geometrical progression" (*loc. cit.*, 60). As, however, his observers had only the intensity of the sound to serve as basis for judgment, it is more probable that the law should read: 'the perceived intensity of the sound diminishes in arithmetical progression when its physical intensity diminishes in geometrical progression.' The results would then be simply a rough confirmation of the validity of Weber's law for sound.

The procedure here indicated is open to the objection that *O* knows the actual distance in every test. We may therefore discard the cage altogether, and either move the single receiver in and out, along a measure, or use two receivers, and allow *O* to estimate the distance of the (singly heard and medianly localised) double click.

If two receivers are used, there are two modes of connection. (1) In series. This is the better way, if the receivers are mechanically and electrically very similar: for precisely the same amount of current goes through the coils. There will still be chance variations in the response of the receiver plates; but these can be minimised — if, indeed, they are noticeable at all — by keeping the diaphragm well away (.75 to 1.0 mm.) from the core of the coil. (2) In parallel. This is the better way if the receivers react unequally, or if *O*'s ears are unequal, since it is possible to put resistance (no. 36 or 40 German-silver wire) in the strong line until the localisation is median.

If clicks and not tones are employed, the rapidity of break must (as was said above) be kept constant. This can best be accomplished by the introduction of an automatic key.

LITERATURE. — M. Matsumoto, Studies from the Yale Psychological Laboratory, v., 1897, 1. Besides the references given by Matsumoto, *cf.* O. Külpe, Outlines of Psychology, 1895, 374 ff.; A. Höfler, Psychologie, 1897, 342.

CHAPTER XI

Tactual Space Perception

EXPERIMENT XXXIII

§ 54. **Localisation of a Single Point upon the Skin.** *Cautions not noted in the Text.* — Avoid temperature points; a vivid sensation of heat or cold is distracting. If a temperature point be touched, record the fact, whether *O* localise or not. But do not use the experiment in drawing your final conclusions; return to the neighbourhood of the point later on in the experimental series.

The length of the interval between experiment and experiment must be regulated according to *O*'s introspections. The after-image of pressure will probably be found to vary in duration for different observers.

It may happen that *O* declares all traces of after-image to have vanished, but that nevertheless a new stimulation serves to bring out a pressure after-image at the spot last stimulated. This means that the skin is not thoroughly rested. The interval between experiments must then be lengthened.

Questions. — (1) By the 'local sign,' which is most probably visual. Outline, 156.

(2) All the later errors are probably smaller than the earlier of the same region. For *practice*, see Külpe, Outlines, 43, 340.

(3) The errors will probably be smaller on the *R* and *U* boundary lines, owing to visualisation; and on the *P* line, owing to visualisation and to an intrinsically lower localisation limen. The skin at the carpal folds is rigidly attached to the underlying tissues; and the wrist is exposed, and so gets more practice than the upper part of the arm.

(4) Different observers give very different results. Since the right hand is localising, and will tend to fall short, there may be

a displacement of all localisations towards the *U* border of the left-arm area. Again: the localisations may be thrown out towards the *R* and *U* borders alike, or thrown up towards the *P* border, owing to visualisation, etc. Cords, scars, etc., may 'attract' localisation towards them, by serving as visual or tactual landmarks.

(5) *a.* Visualisation is not excluded. Hence the experiment is not a pure tactual experiment.

b. The errors are not, as they stand, to be counted as errors of localisation. Chance must be taken into account. See W. B. Pillsbury, Amer. Journ. of Psych., vii., 1895, 42.

c. The units of direction are rough. Errors do not all fall upon the eight radii employed.

(6) Experiment upon the right arm, to see whether the localisations are shifted towards the *R* side of the area. Experiment upon the back of the neck, visualisation of which is not so accurate; or upon any part of the body which has not clear visual boundary lines.

Related Experiments.—This experiment is capable of many variations, all of which are of interest for the theory of tactual localisation. Thus *O* may localise the point of impression without himself touching his arm: he arrests the point of his pencil in the air, over what he takes to be the point of stimulation. *E* then drops a perpendicular to the arm, and measures the amount and direction of the error of localisation (Henri, 100; C. S. Parrish, Amer. Journ. of Psych., viii., 1897, 250). Or *O* may open his eyes, after the arm has been stimulated, and mark the point of stimulation with his pencil upon a life-size photograph or plaster of Paris model of his arm (Henri, 117; W. B. Pillsbury, Amer. Journ. of Psych., vii., 1895, 55). Or he may look at his arm while *E* is stimulating it, and then close his eyes, and localise the point in the usual way with his own pencil (Henri, 106; Pillsbury, 44, 46). Or he may follow the ordinary method, but make a special effort to suppress or to reinforce visualisation during the time that *E* is holding the point upon his arm (Henri, 98; Pillsbury, 46, 51).

Literature.—The method followed in this Experiment is known as 'Weber's Second Method': see E. H. Weber, Ueber

den Raumsinn, etc. (Verh. d. k. sächs. Ges. d. Wiss., math.-phys. Classe), 1852, 89 f. *Cf.* also J. Czermak, Physiol. Studien, ii., 1855, 52 f.; W. B. Pillsbury, Amer. Journ. of Psych., vii., 1895, 42; W. Lewy, Zeits. f. Psych., viii., 1895, 254; V. Henri, Ueber d. Raumwahrnehmungen d. Tastsinnes,[1] 1895, 90 ff. (esp. 102 ff.); Sanford, Course, 2, exp. 2; C. H. Judd, Philos. Studien, xii., 1896, 411 f.

For and against local signs, see Wundt, Phys. Psych., ii., 1893, 36 ff., 215 ff., 231 ff.; Outlines, 105, 127 ff., 134 ff.; Külpe, Outlines, 344, 369; James, Psych., ii., 155 ff., 167 ff.; Stumpf, Ueber d. psychol. Ursprung d. Raumvorstellung, 1873, 106 ff., 272 ff.; T. Lipps, Psychol. Studien, 1885, 1 ff.; Grundtatsachen d. Seelenlebens, 1883, 472 ff.; Henri, Raumwahrnehmungen, 159 ff.; Hering, Beitr., v., 1864, 323; Hermann's Hdbch., iii., 1, 1879, 565, 572; J. Ward, art. Psychology, Encycl. Brit., 9th ed., 1886, 46, 53. For R. H. Lotze's theory of local signs, see the Medicinische Psych., 1852, 325 ff.; the appendix to Stumpf's Ursprung, etc.; and the references in Henri, 177.

These references are given rather for the Instructor than for the student. The road of space theory is, as we said above (p. 256), rough travelling for the beginner in psychology; and the question of nativism *vs.* genesis may well be postponed until the conclusion of this Course.

EXPERIMENT XXXIV

§ 55. **The Discrimination of Two Points upon the Skin.** *Cautions not noted in the Text.* — The cautions of Exp. XXXIII. must be regarded with great attention here. Disturbance by temperature spots must be noted, but is not a reason for repeating an experiment. Temperature difficulties may, however, justify a slight travelling out of the line, in the lateral direction. *Cf.* G. A. Tawney, Philos. Studien, xiii., 1897, 169. — The intervals between experiments must, naturally, be longer than they were in Exp. XXXIII.

[1] This work has a bibliography of 322 titles.

Tawney recommends an application time of 4 sec. and an interval of 10 to 20 sec. (165 f., 173). C. H. Judd (Philos. Studien, xii., 1896, 417) advises 3-sec. applications. These application times are, in the author's judgment, needlessly long.

It is important to keep the temperature of the room constant: E. Loewenton, Versuche üb. d. Gedächtniss im Geb. d. Raumsinnes d. Haut, 1893, 18; Tawney, 166.

The questions of method (see References below), of the time, vacillations and relative certainty of judgment (Tawney, 194), of the direction of attention (194, 210 f.; Judd, 429), and of the existence of two limina (Tawney, 174), are all weighty questions. They cannot, however, be discussed in this volume.

Note that the æsthesiometer is so constructed that the limbs are always vertical to the cutaneous surface (Tawney, 164).

RESULTS. — The following is the first set of results obtained from a careful *O*.

Left fore-arm, volar surface, ⟷

Mm. ↓	Jdgt.	Jdgt.	Mm. ↑
24	2	—	—
23	2	—	—
22	2	2	22
21	2	2	21
20	1	2	20
19	1	2	19
18	1	?	18
—	—	1	17
—	—	1	16

The determination of the average value is here very easy. We take the mean of 20 (the first 'One' of ↓) and 19 (the first 'Two' of ↑). The value is, therefore, 19.5 mm.; it will decrease with practice. The effect of the error of expectation is shown, though not clearly. There is no introspection, over and above the words 'One,' 'Two,' and the 'Don't know' of exp. 3 ↑.

The following is a series taken from the same *O* at a more advanced stage of practice.

Left wrist, volar surface, ⟷

Mm. ↓	Jdgt.	Jdgt.	Mm. ↑
30	2 (quickly)	—	30
20	"	—	20
15	"	—	15
14	"	—	14
13	"	—	13
12	"	2 (certain)	12
11	2 (with hesitation)	2	11
10	"	Spread out, almost like 1	10
9	At first 2, then 1	2	9
8	Suggestion of 2; they seemed to run together.	2 (with hesitation)	8
7	Line or oval; 1	1	7
6	1	1	6
5	1	1	5
4	1	—	4

The ↓ series is unnecessarily long; the exps. with 9 and 8 mm. show the error of habituation. A shorter series would have given the values 10 or 9 and 8 mm., instead of 7 and 8 mm. The introspections show great improvement.

Henri (6) gives the following introspective stages for an ↑ series:

(*a*) One small sharp point;
(*b*) a larger, blunter point;
(*c*) a small area of oval form;
(*d*) a line;
(*e*) two points, near together, connected by a line of light contact;
(*f*) two separate points; direction of the line of junction uncertain;
(*g*) two separate points; direction known.

This is, of course, an ideal series. See, further, G. A. Tawney, Psych. Rev., ii., 1895, 587 ff.; Philos. Studien, xiii., 1897, 174, etc.; C. H. Judd, *ibid.*, xii., 1896, 428.

The perception of two separate points, while the judgment of direction is still uncertain, belongs to a group of phenomena

which have often been discussed in experimental psychology, but which still await systematic treatment. See Judd, 419, 423, 430 f., 436 f. According to G. S. Hall and H. H. Donaldson, movement over the skin is perceived before the direction of movement can be given (Mind, O. S., x., 1885, 557); *cf.* James, Psych., ii., 172 f. James remarks, again, that "difference, . . . immediately felt between two terms, is independent of our ability to identify either of the terms by itself," and posits "a real sensation of difference" (i., 495 f., and references). A. Goldscheider notes (Ges. Abh., ii., 196) that movement of a limb may be perceived before there is any certainty as to the direction of its movement. Külpe refers these cases to the operation of "a psychological law absolutely valid within certain limits, — the law that general denominations are more easily reproduced than special" (Outlines, 172). This law is "itself only a particular case of the universal rule that the frequency of excitation exerts an influence upon the reproductivity of impression. . . . When memory begins to fail in consequence of age, concrete names . . . are forgotten before abstract. . . . The existence of a difference between the compared sensations is earlier remarked than the direction which it takes, or the nature of the objects between which it obtains" (173 f.). The hypothesis of specific 'movement sensations' and 'difference sensations' is discussed and rejected, pp. 347 f. Finally, Külpe brings under the same heading of 'reproduction of the general' many of the results of experiments upon the duration of the association (association-reaction).

Külpe's law is undoubtedly valid in many cases. But the facts need sifting. A small coloured surface, seen from a sufficient distance, looks colourless (see Sanford, Course, 142, exp. 143). Is this merely because the abstract term 'light' is more easily reproducible than the concrete term 'red' or 'blue'? On the other hand, Wundt explains the confusion of very weak warm sensations with minimal pressure sensations by their reference to a single sense-organ (Phys. Psych., i., 1893, 416): so that the law is not confined to the domain of perception, but has an application in the sphere of sense. The whole matter calls for reinvestigation, more especially in the light of Meyer's recent

statement (Zeits. f. Psych., xvi., 1898, 359 ff.) that the time required for the cognition of tonality is also sufficient for the cognition of determinate pitch.

QUESTIONS. — (1) For expectation, see Külpe, Outlines, 39. Let *O* write out an introspective analysis of the state. If the tendency to correct were present, the fact must be noted: the results will be variable and unreliable.

(2) Habituation (Külpe, 41), the tendency to have now the same perception that you have had before. Fatigue (Külpe, 43).

(3) In order that the conditions may be kept constant. The degree of habituation and fatigue, and the amount of practice and expectation, with which the region of change is approached, should evidently be the same for both series.

(4) *O* may have been inattentive, tired, habituated, etc. *E* may have made the series too long, have waited too long between separate experiments, have made the waits irregular, etc.

(5) One has no right to argue from the results of a given method to that of a single experiment taken by a different method. We have determined the required distance by creeping up to it, from two different positions, cautiously and by slow degrees. We have used a *gradation* method, a method of steady approach by small steps. When the compasses are set down once and for all, as the Question supposes, we have travelled into an *error* method. In an error method, a single distance would be set down, over and over again, and introspective 'shots' taken at it. Notes would be made of the number of cases in which there was a perception of two points, and of the number in which there was a perception of one. From these data we could calculate out the distance required. But it is unfair to take a single trial: the error methods demand a large number of trials. The careful stalk may not be more successful than a multitude of 'shots'; we have no right to compare it with a single shot. — These considerations show us an important truth: the truth that an experiment which aims at a quantitative result is never a detached trial, capable of being performed by itself without respect to other experiments, but is always an experiment within an experimental method, *i.e.*, a detached member of a systematised series of experiments.

In actual fact, the probabilities are (the arm being tired) that the two impressions would give rise to a single perception.

(6) Between the perceptions of two points and of one point, there will probably be perceptions of line or blur, due to summation and irradiation of sensations from the pressure spots.

(7) Variation: work with larger or with smaller steps. Modification: reduce your steps to the smallest size possible when you approach the region of change.

(8) Work crosswise on the arm, because (1) you will then get the effect of visualisation, and (2) the pressure spots are themselves more thickly distributed transversely than they are longitudinally: A. Goldscheider, Neue Thatsachen über die Hautsinnesnerven, Arch. f. [Anat. u.] Physiol., Suppl. Bd., 1885, 100; *cf.* Tawney, Philos. Studien, xiii., 170; Judd, *ibid.*, xii., 425. Work upon forehead, tip of forefinger, ball of thumb.

(9) Since practice would aid discrimination, the distance-values of the two points of change would both be rendered smaller. The dotted line of the Fig. would be shifted downwards, parallel with itself. — In the single experiment, practice will affect the ↑ more than the ↓ series. The divergence of the dotted line from the horizontal will therefore be increased.

(10) To avoid complication by pain and temperature sensations.

(11) Because it is always best to start out with something that is quite clear and easy, and to work from that towards the obscure and difficult. Since *O* knows that in all cases there are two points set down upon the skin, he is likely to be puzzled if you begin with a two-point impression which is perceived only as one point. — If the experiment is repeated, and practice is gained, the series should be alternated; the first experiment ↓ ↑, the second ↑ ↓, the third ↓ ↑ again, etc.

After determinations have been made in the transverse direction of the arm, ask *O* and *E* why they were directed to begin with experiments on the longitudinal axis. The reason is that the arm taken lengthwise localises more roughly than the arm taken crosswise; and, when you are beginning a line of work, it is better to operate with the rougher of two available machines. Your own mistakes will be liable to do less harm.

RELATED EXPERIMENTS. (1) *The Paradoxical Localisation Experiment.* — It not infrequently happens that single impressions give rise to double perceptions. The pressure of one limb of the compasses, *i.e.*, may be perceived as that of two distinct points. Give irregularly alternating series of two-pressures and one-pressure. Note: (1) the influence of *expectation* on the number of wrong double perceptions in a given series; (2) that of *fatigue;* (3) that of the separation of the compass points in the experiments in which two points are really set down. Notice also (4) the distance at which the supposed second pressure seems to lie from the given single pressure; (5) its direction; and (6) its character (intensity, extent, duration) as compared with that of the given pressure.

You will find that expectation and fatigue increase the number of double perceptions; that wide separation in the alternate experiments decreases it; and that the character of the supposed second point varies. Sometimes the real and supposed points are connected in introspection by a line of pressure. — The conditions of the second perception are probably physiological, not psychological, though the frequency and insistency of it are modified by *O*'s frame of mind.

Henri, 61–66, and references; esp. Tawney, Philos. Studien, xiii., 197, 220; H. Nichols, Our Notions of Number and Space, 1894, 161.

(2) *The Localisation Pattern.* — Make some rough trials with the compasses on the volar side of the upper arm, just above the elbow-crease, and in the transverse direction. Give the points a separation which is a little less than that required for the arousal of two perceptions, and draw the compasses slowly and steadily down the arm to the tips of the second and third fingers. Mark the resulting series of perceptions upon an outline map of the arm, and (for the sake of comparison) rule on the map in dotted lines two parallels, corresponding to the track of the compass points. The perception-figure will shrink to a single line on parts of small discriminating power, and widen out into loops on the regions of greater discrimination.

Perform the same experiment on the face. Start upon the cheek-bone, just below the lobe of the ear, and draw the com-

passes transversely, so that the points pass above and below the red portions of the lips. Mark the resulting series of perceptions on a map.

E. H. Weber, Ueb. d. Raumsinn, etc., 93; Henri, 58 f.; Sanford, Course. 4, exp. 7 *c*; Judd, 456 f.

INSTRUMENTS.—There are many forms of æsthesiometer, which we shall describe in vol. ii. Fig. 101 shows H. Griesbach's dynamometrical æsthesiometer (Brändli, $17). This instrument is held by the thumb and the first and second fingers of *E*'s right hand. It has a mm. scale and nonius. The points (of which there are four, — two rounded and two pointed) push against coiled springs, and a scale-pointer indicates the amount of pressure exerted. The points are of metal. This is a disadvantage, as it introduces the temperature error. Pflüger's Arch., lxviii., 1897, 65.

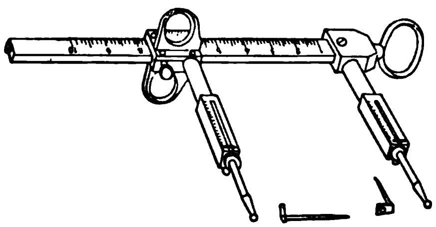


FIG. 101.

Note that (as the author found in 1892, and as Tawney has also discovered: Philos. Studien, xiii., 168) equality of objective pressure-intensities by no means guarantees equality of subjective pressures. The compass points may be 'pressing equally' while the two sensations aroused are markedly different in intensity. The important thing is to have the two pressure sensations clear and distinct (Tawney, 168 f.; F. B. Dresslar, Amer. Journ. of Psych., vi., 1894, 331).

LITERATURE.—The experimental literature begins with E. H. Weber (Annotationes anat. et physiol., 1834, 44). The Table of values in the article on Tastsinn und Gemeingefühl (Wagner's Handwörterbuch d. Physiol., iii., 2, 1846, 539) has often been quoted in textbooks of psychology. On the method of the text (a form of Weber's 'First Method'), see Külpe, Outlines, 55 f.; Henri, Raumwahrnehmungen, 12; Tawney, Philos. Stud., xiii., 173. For a general account of work done upon the 'Raumsinn d. Haut,' *cf.* Henri, 5 ff.

On visualisation, see M. F. Washburn, Philos. Studien, xi., 1895, 190; on the method of successive stimulation, C. H. Judd,

ibid., xii., 1896, 415; on linear stimulation, Judd, 413, 431; Goldscheider, Arch. f. [Anat. u.] Physiol., 1885, Suppl. Bd., 84, 95; C. S. Parrish, Amer. Journ. of Psych., vi., 1895, 520.

EXPERIMENT XXXV

§ 56. **Localisation with Changed Position of Parts Stimulated.** *Cautions not noted in the Text.* — It is very important, for the success of this experiment, that all the directions should be carefully carried out. The ink-dots mentioned on p. 191 of the text should be placed on the *R* side of the second and the *U* side of the third finger, and should always touch when the fingers are crossed. In the 'normal' position of the fingers, the *U* side of the second finger is apposed to the *R* side of the third, along the whole length of the latter. The student must plainly understand that two impressions constitute a single experiment in series (1), and that only one impression is required in series (2). Be very careful that the impressions last for at least 1 sec., and are not of the nature of dabs or taps upon the skin. The crossing and uncrossing of the fingers should be done in a business-like and matter-of-fact way, so that *O*'s attention is not particularly called to the proceeding. *E* should handle *O*'s fingers as little as is compatible with avoidance of the fatigue which naturally results in time from the strain of the crossed position. The 'crossed' series should directly follow the 'normal' series in each of the three cases. The hand experimented on must lie upon the table as flat as it comfortably can; the upward bend of the finger-tips, the 'give' when the compass points are set down, must be reduced as far as possible. The compasses must be held vertically, and not approach the skin obliquely.

RESULTS. — The results are as follows. In Aristotle's experiment, one object is perceived as two. In the normal discrimination series, the 20 mm. separation is the clearest; in the crossed series, the reverse obtains. A similar result, though of less pronounced a kind, is gained with the two distance series. The estimated distances are fairly correct; correct in the normal series, correct after reversal in the crossed series. The direc-

tions are rightly given in the normal localisation series, and are exactly reversed in the crossed-finger series.

QUESTIONS. — (1) In most cases, *O* is sure of his results: surprisingly sure, when the character of these results is considered. Uncertainty may arise from lack of practice, or from similarity of stimuli (thus the 20 and 12 mm. separations may occasionally be only doubtfully distinguished). A moderate degree of uncertainty, varying with individuals, may attach to the estimations of series (2): the results of this series are, as a rule, less satisfactory in the case of beginners than are those of series (1) and (3).

(2) When we try to account for the results, we have to note first of all what are the psychological factors involved. These seem to be three in number. We have (*a*) the 'local signs' of the two skin areas (see Exp. XXXIII.). These are probably visual. We have (*b*) the reinforcement of the local signs by our recent visual experiences. In other words, we have a number of visual memories, more or less fresh, which tell us that in everyday life the two skin areas upon which we are experimenting are not touched except by two different objects. And we have (*c*), making against these two factors, our present knowledge, visual and tactual, of the position of the fingers. Just as present knowledge, in Aristotle's experiment, may counteract the local-sign evidence from the fingers, so might we expect that in these later series of experiments our knowledge of finger-crossing would counteract the reversal of the local signs and lead to the construction of new 'right' and 'left,' and of a new scale of distances.

It is evident from the results, however, that factors (*a*) and (*b*) are (with occasional exceptions in the distance experiments) fully able to outweigh factor (*c*). The old visual-tactual relations persist in the new position of the fingers; an objective 'left' in this position is taken to be 'right,' because it would be 'right' in the normal position; an objective 'long' is taken to be 'short,' because it would be 'short' in the normal position; etc. The general explanation asked for is, then, that, no matter how the normal relation of the two stimulated surfaces is changed, there is constancy of tactual localisation.

(3) Because the forefinger has no finger apposed to it on its *R* side, and is thus differentiated from the second and third fingers. Nevertheless, the results gained can be verified (although, perhaps, not quite so easily as they were gained) with the fore and middle, or third and fourth fingers. — The second is the longer finger.

(4) Just the same results are obtained. The only change in the conditions of the experiments is that *O*'s present visual and tactual knowledge of the position of the fingers (dorsal side up) is not identical with their position on the maps (volar side up); and this change is not important enough to confuse his localisations.

(5) We are constantly manipulating objects with our fingers in our daily life, and so come to have an accurate visual idea of the width of these phalanges. This visual idea is easily translated into the visual pencil-line that the experiment calls for.

(6) Experiments with other fingers have been mentioned above. The right hand might be worked on. Bring together the tip of the nose and the upper lip, or lay the lobe of the ear against the head, and touch the two apposed surfaces lightly with a pencil or the whalebone; you get the perception of two objects. Draw the two lips apart sideways, the one to the left and the other to the right. Apply the compass points vertically, the one to the upper and the other to the lower lip. The two impressions seem to lie diagonally, and in an opposite direction to that in which the lips are drawn.

(7) A visual memory-image of the lines drawn in previous experiments would form part of *O*'s consciousness at the moment that the new impression was given. The line next drawn would, therefore, be the line associated not to this tactual perception alone, but to the tactual perception *plus* the memory-images.

On suggestion in general see J. M. Baldwin, Mental Development (Methods and Processes), 1895, 104 ff., with references; Wundt, Hypnotismus u. Suggestion, Philos. Studien, viii., 1892, 1; Lipps, Suggestion u. Hypnose, eine psych. Untersuchung, 1895 (Sitzungsber. d. p.- p. u. d. hist. Cl. d. k. bayer. Akad. d. Wiss., ii., 1897, 391); Pillsbury, Amer. Journ. of Psych., viii., 315; W. C. Bagley, *ibid.*, xii., 1900, 80; A. Binet, L'Année psych., v., 1899, 82.

Related Experiments. — In the two experiments now to be described, local sign and present visual knowledge are set more

nearly upon an equality than they have been in the previous experimental series. The second experiment, moreover, allows one, with practice, to dissociate the two factors entirely.

(1) Extend the two arms, turning the volar surfaces of the hand outwards. Lay the hands together, right over left, apposing the volar surfaces. Link the fingers of the two hands. Bend the elbows, and bring the hands up, opposite the chest, fingers upwards. As you look down on the hands, the fingers of the right hand extend from left to right, those of the left from right to left. *E* now points to a finger, without touching it; and you try to move it. As a general rule, you move the corresponding finger of the other hand.

Repeat the experiment, allowing *E* to touch the finger which he wishes you to move. What happens? — Henri, 139.

(2) *E* provides a sheet of paper, upon which two points have been marked in a diagonal direction; a screen of paper or cardboard; a pencil; and a mirror. *O* seats himself before the mirror, the pencil in his right hand. The paper is laid down before the mirror, and the screen so arranged that *O* cannot see the paper directly, though he can see its reflexion. The point of *O*'s pencil is set by *E* upon one of the two points, and *O* endeavours to draw a straight line from that to the other point, guiding his movement by its reflexion in the mirror. Individual differences are large: usually, however, *O* finds the task difficult, confusing up with down and right with left. Practice removes the difficulty. — Henri, 139 f.

LITERATURE. — Aristotle's references to the illusion of duality are to be found in the Περὶ ἐνυπνίων, ch. 2, 460, and in the Metaphysics, iii., ch. 6, 1011 and x., ch. 6, 1063. The present Experiment is based upon Henri's investigation: Raumwahrnehmungen, 69 ff., 136 ff. *Cf.* also J. Czermak, Physiol. Studien, ii., 1855, 91 ff.; G. C. Robertson, Mind, O. S., i., 1876, 145; W. H. R. Rivers, Mind, N. S., iii., 1894, 583.

On the value of Aristotle's experiment in systematic regard, see Henri, 169, 192, 195, 208, 212.

CHAPTER XII

Ideational Type and the Association of Ideas

EXPERIMENT XXXVI

§ 57. **Ideational Types.** — The author has chosen the phrase 'ideational types' or 'types of idea' — in accordance with the historical usage of English psychology — to denote what are more generally termed 'memory types,' 'speech types,' 'types of imagery.' The psychology of these types begins with the independent work of G. T. Fechner, F. Galton and J. M. Charcot.

Fechner (Elem. d. Psychophysik, ii., 469 ff.: On the Interrelation of Memory Images and After-images) publishes introspective accounts of the visual imagery of several well-known persons, and gives a very elaborate comparison of his own memory-images and after-images. The programme which he drew up for further work was carried out, with curious exactness, by Francis Galton (Inquiries into Human Faculty, 1883, 83 ff.; *cf.* articles in Mind and The Fortnightly Review for 1880). Charcot came to the question from the pathological side (see Binet, The Psychology of Reasoning [1886], trs. 1899, 13; Charcot, Leçons sur les maladies du système nerveux, Oeuvres complètes, 1886–1890, t. iii. [trs. by S. Freud, Neue Vorlesungen üb. d. Krankheiten d. Nervensystems, 1886]).

The best general account in English is that of James: Principles, ii., 50 ff. See also L. W. Stern, Ueber Psychologie d. indiv. Differenzen, 1900, 47 ff. (bibliography, 138 f.); W. Lay, Psych. Rev. Mon. Suppl. 7, 1898; H. Taine, De l'intelligence, i., ed. of 1883, 76 ff.; T. Ribot, Les maladies de la memoire, ed. of 1891, 106 ff.; J. M. Baldwin, Mental Devel. in the Child and the Race, 1895 (1899), 431 ff. and references; and the author's Outline of Psych., 285 ff., 293; Primer, 123 ff.

The psychological questionary seems to have originated with Galton. The method has, of late years, been widely used by G. S. Hall and his pupils at Clark University (see recent vols. of the Amer. Journ. of Psych.).

Question (1). — The requirements are given by Galton (84) as follows. (*a*) The questions must be such as will be quickly

and correctly understood. (*b*) They must admit of easy reply. (*c*) They must cover the ground of enquiry. (*d*) They must "tempt the correspondents to write freely in fuller explanation of their replies, and on cognate topics as well." "These separate letters," says Galton, "have proved more instructive and interesting by far than the replies to the set questions."

It is plain that the drawing-up of a questionary is no light task. The questioner must, in the first place, know the ground that he desires to cover. He must, secondly, be a master in the art of questioning itself. And, thirdly, he is called upon to exercise sound judgment in the acceptance or rejection of individual replies. What, now, can the questionary accomplish? What does it do that could not be done by the personal questioning of a few individuals?

In the hands of a capable enquirer, it can accomplish three things. (*a*) It can serve to establish a norm. If the answer to a given question is, in a very large number of cases, essentially the same, we can accept it as an average or normal account of the phenomena under investigation. The collective result commands a higher measure of belief than the single result, because it is not likely that 50 or 100 independent observers, all of whom are liable to error, will all be liable in extreme degree to the same error. (*b*) It can give a fuller characterisation of the phenomena than can the single description. Suppose that a number of persons are reporting the same occurrence, and that there is no contradiction between report and report, but that there are differences of detail, — one report emphasising one of the minor features of the whole, and another another. We get from a combination of all the reports a fuller and more accurate picture than we could get from any one taken singly; the separate accounts not only reinforce but also supplement one another. (*c*) It can bring out individual differences. Suppose that, while the majority of the answers are in essential agreement, there are a certain number — returned by persons of similar education and equal trustworthiness — that agree among themselves but differ radically from the rest. We are justified in accepting this smaller group, at least provisionally, as evidence

of the existence of a second 'type' or 'norm' and in making it the starting-point of further investigation.

All three results were accomplished by Galton's questions upon Visualising and Other Allied Faculties (Inquiries, 378 ff.), which may be said, as James remarks, "to have made an era in descriptive psychology." But we must not forget the limitations of the questionary. Galton writes: "It is a much easier matter than I had anticipated to obtain trustworthy replies to psychological questions. Many persons . . . take pleasure in introspection, and strive their very best to explain their mental processes. I think that a delight in self-dissection must be a strong ingredient in the pleasure that many are said to take in confessing themselves to priests" (87). And James gives us the same idea when, after declaring that "Fechner was gifted with unusual talent for subjective observation," he refers to Fechner's comparison of memory-images and after-images as "a type of observation which any reader with sufficient patience may repeat" (i., 50 f.). Such statements suggest that introspective exercises may be paralleled, as a form of polite recreation, with the word-puzzles in the magazines, and that the circular of questions is a royal road to the attainment of psychological truth. Nevertheless, Galton asserts that "there is hardly any more difficult task than that of framing" a successful questionary (84); and James prints his own introspective results with great modesty and reserve (65).

There is here a real contradiction, but a contradiction which it is not difficult to resolve. We must remember that the questionary can never transcend or go behind the introspections of the individual correspondents. The collective result is worthy of more credence than the individual result, but it does not penetrate more deeply than this into the structure of mind. Stumpf's mass-tests of the relative unitariness of chords serve to establish the scale of fusion degrees, but tell us nothing of tonal analysis. So the questionary-returns upon visual imagery enable us to plat our curves of distribution, but tell us nothing more of the mechanics of visual thinking. We put point-blank and clear-cut questions: questions which, in their ideal form, demand no more than a 'yes' or a 'no' for answer: and we get replies upon the introspective level of the average educated man. This level is low. It remains low, even when the introspections are directed and assisted by our phrasing of the questions. The self-dissection of the confessional and of the 'psychological novel' is, therefore, a very different matter from the introspection of psychological science; and the 'patient reader' will have but small success if he try, without practice or model, to match the analyses of Fechner and James.

Galton's work lay, if we may use the expression, on the objective side of psychology; and this fact accounts for its success. The ordinary observer, untrained in psychological method, can give an opinion as to the match of two colours upon the colour mixer, while he is wholly unable to follow the course of an after-image. Similarly, the ordinary observer can tell us whether his

mind is furnished with visual ideas, and can describe some of the uses to which he puts his visual furniture in everyday life (Galton, 95 f.), though he will be wholly unable to unravel the part-processes in visual recollection, visual recognition, visual apperception, etc.

We may cite, in support of the position here taken, the elaborate monograph of E. D. Starbuck, on The Psychology of Religion; an Empirical Study of the Growth of Religious Consciousness (London, 1899). It has been pointed out by a reviewer (J. H. Leuba, Psych. Rev., vii., 1900, 515) that phrases like 'a vital experience of spiritual truth,' 'the attainment of spiritual life,' 'a spiritual grasp, a new insight,' 'the higher life of intelligence and insight,' 'a personal hold on virtue,' 'a first-hand perception of right and wrong,' are hardly in place in the work of a professional psychologist. Yet they fairly represent the introspective depth to which the writer's questionaries have taken him.

However, the fault lies in such cases not with the method, but with those who overestimate the method. On this score, as we have seen, both Galton and James are to be held guilty. Let us now enquire into the scope of the questionary, in the various fields of psychological research.

The questionary does excellent psychological service (*a*) in a field of observation which is, in strictness, extra-psychological: that of expressive bodily movement. Darwin made use of it in getting material for his work on the Expression of the Emotions in Man and Animals (1872). The applicability of the method in this sphere, and its importance for animal, infant and ethnic psychology, are obvious, and need not be insisted on. (*b*) The questionary can inform us of the variation of a given mental fact with variation of age, sex, race, occupation, etc. It thus throws light upon what we may call the 'natural history' of mind. *Cf.* Galton's statement that "the power of visualising is higher in the female sex than in the male" (99). And (*c*) the method is of value in purely descriptive psychology, where the introspection required is of a simple and 'massive' kind. Here belong the major part of Galton's enquiries, and such an enquiry into the types of ethical and religious sentiment as is suggested in the author's Outline, 334 f.

We are ourselves employing the method under extremely favourable conditions, since the *O*'s to whom the questions are submitted have already had training in introspection. Under such circumstances, the individual results have a higher value than usually attaches to the single sheet of questionary-returns.

Questionary upon Ideational Type. — This questionary includes practically all of Galton's questions, and is also indebted for special points to a Wellesley questionary upon Memory Type and to the Psychol. Schulversuche of Höfler and Witasek, 14 f. (cube and octahedron). It is in so far an improvement upon Galton's paper as that the questions upon auditory, tactual, etc., imagery are drawn from definite situations, and not made a mere appendix to the visual portion of the enquiry. For somewhat similar lists, see Lay, 21; R. H. Stetson, Psych. Rev., iii., 1896, 402.

The questionary is still most complete and satisfactory upon the visual side. "A statistical enquiry upon a large scale, into the variations of acoustic, tactile, and motor imagination, would probably bear less fruit than Galton's enquiry into visual images" (James, ii., 65). The reason is, simply, that the demands upon introspection are greater.

On the auditory type, and questions pertaining to it, see B. Bourdon, Ber. über d. 3 internat. Congress f. Psychol., 1897, 240 f.; Stern, 53; Stumpf, Tonps., i., 279 ff.; A. Binet, Psychologie des grands calculateurs et joueurs d'échecs, 1894, 24 ff.

On the tactual or 'motor' type and its interrelations, see Stricker, Studien über d. Sprachvorstellungen, 1880; Studien über d. Bewegungsvorstellungen, 1882; Studien über d. Association d. Vorstellungen, 1883; Rev. philos., xviii., 1884, 685; Stumpf, Tonps., i., 153 ff.; F. Paulhan, Rev. philos., xvi., 1883, 405; xix., 1885, 118; xxi., 1886, 26; R. Dodge, Die motorischen Wortvorstellungen, Halle, 1896.

Results. — Each student enters his own questionary-returns in his note-book, and then hands the sheets to the Instructor for statistical working-over. The returns from the class should be arranged according to Galton's classification (Human Faculty, 49 ff., 93). The following is Galton's specimen Table. Similar Tables are to be made out by the Instructor (or by some student deputed by him to the task) in the other sense-departments. All should be recorded in the note-books, so that every student knows his place in the various scales.

It is well to save the answer-sheets, from year to year, and to give them to the students for working-over, after the questionary itself has been answered. Practice is thus gained in the manipulation of statistical results.

Galton's Table for Vividness of Mental Imagery.

Highest. — Brilliant, distinct, never blotchy.

First Suboctile. — The image once seen is perfectly clear and bright.

First Octile. — I can see my breakfast-table or any equally familiar thing with my mind's eye quite as well in all particulars as I can do if the reality is before me.

First Quartile. — Fairly clear;. illumination of actual scene is fairly represented. Well defined. Parts do not obtrude themselves, but attention has to be directed to different points in succession to call up the whole.

Middlemost. — Fairly clear. Brightness probably at least from one-half to two-thirds of the original. Definition varies very much, one or two objects being much more distinct than the others, but the latter come out clearly if attention be paid to them.

Last Quartile. — Dim, certainly not comparable to the actual scene. I have to think separately of the several things on the table to bring them clearly before the mind's eye, and when I think of some things the others fade away in confusion.

Last Octile. — Dim and not comparable in brightness to the real scene. Badly defined with blotches of light; very incomplete; very little of one object is seen at one time.

Last Suboctile. — I am very rarely able to recall any object whatever with any sort of distinctness. Very occasionally an object or image will recall itself, but even then it is more like a generalised image than an individual one. I seem to be almost destitute of visualising power as under control.

Lowest. — My powers are zero. To my consciousness there is almost no association of memory with objective visual impressions. I recollect the table, but do not see it.

The Instructor should be on the look-out for special remarks bearing upon peculiarities of mental constitution. Nearly every paper will contain some such observations. Here are two, from the first two sheets of the author's pile of class returns.

(1) If I am tired, I generally find that mathematical work is made easier if I visualise. I always feel as if I am working more slowly when I visualise, but it frequently makes things clearer.

(2) I always think of numerals as printed in rather heavy-faced type. I almost always have images without colour. In fact I always dream in black, white and grey.

If a student is noticeably weak in one or other of the partial memories, he may be advised to practise it, with a view to improvement. See Galton, 105 ff.; James, ii., 58, 60; E. B. Tal-

bot, Amer. Journ. of Psych., viii., 1897, 414; Cohn, Ber. üb. d. 3 internat. Congress für Psych., 1897, 458; Stetson, *loc cit.*, 408.

As to the relative frequency of the various types of idea, there seems to be no doubt that the great majority of students are predominantly visual (*cf.* A. C. Armstrong, Psych. Rev., i., 1894, 505). Galton says (87) that "scientific men, as a class, have feeble powers of visual representation." It must be remembered, however, that the men here referred to are men of standing and reputation, — men, therefore, who have lived long enough to outgrow their visualisation. Unless the visualising powers are kept alive by occupation (*cf.* Hankel's case in Fechner, ii., 480, 487 f.), there is a tendency in 'thinking' minds for the visual to be replaced by a verbal-motor imagery. — *Cf.* Lay, 15 f.

The verbal-motor type stands, in the author's experience, next in order of frequency to the visual. How common this type is in general society, or in the less educated classes of the community, cannot be said. It is probable, however, that a general enquiry, if it could be carried out, would reveal a very large preponderance of visualisation.

The auditory type is rare, except in the form of verbal auditory-motor. The questionary may bring home to the student his lack of purely auditory images, and so serve as incentive to the study of music: a result devoutly to be wished (*cf.* p. 52 above). See Binet, Psychology of Reasoning, 22 f.

It is worth noting that smell images, which Wundt declares to be extremely uncommon ("It is in most cases illusion when you think that you can recall the scent of a rose": Human and Animal Psych., 286), have been found by Dr. Gamble to be of fairly frequent occurrence. "Allowing for the untrustworthiness of my introspective returns," Dr. Gamble writes, "and although I do not have smell memory-images myself, I cannot think that they are as rare as you say. Fully half of our 65 subjects insisted that they had them. . . . Moreover, there was a clear tendency in the limina of these subjects to be lower than the limina of the other subjects. Which fact was cause and which effect I do not know; but the coincidence was marked." *Cf.* Lay, 37.

On the 'organic,' or as it is also called the 'affective,' memory type, see T. Ribot, The Psychol. of the Emotions, trs. 1897, 140 ff.; Titchener, Phil. Rev., iv., 1895, 65; Outline, 292 ff.; Primer, 129 f.; Lay, 38 f.

Question (2). — There are several methods of determining ideational type, and of attacking the problems connected with

it. Those with which the author is acquainted are given below. It is desirable that the student, besides answering the questionary, should work by one or other of the experimental methods here outlined. The author advises that the various methods be distributed to various pairs of students, and that the results of the whole class be worked over by the Instructor, so that a general statement, prepared by him, may be copied into all notebooks.

(1) The most obvious, and perhaps the most reliable, method of gaining information about ideational type is to read "monographs, by competent observers, about their own peculiarities." We have material of this kind from Fechner, Stumpf, Stricker, James and others. See Lay, 32, 36, 40; Dodge, *op. cit.*

(2) *The Word Method.* — This method, like the questionary, can be employed in cases where there has been no previous training in introspection. It has two forms.

(*a*) *Kraepelin's Method.* — Require *O* to write out a list (i) of objects that are characterised by their colour and (ii) of objects that are characterised by their sound. Let him have 5 min. for each list. — E. Kraepelin, Psychol. Arbeiten, i., 1895, 73; G. Aschaffenburg, *ibid.*, i., 255.

(*b*) *Secor's Method.* — A series of (say, 20) words is written or printed upon slips of paper. At the "Now!" *O* glances at a word, and at once notes down (i) the nature of the verbal image (visual, auditory, articulatory) and (ii) the further imagery suggested by the word as seen.

The experiment is repeated with a second series of words, which are read, not shown, to *O.* — W. B. Secor, Amer. Journ. of Psych., xi., 1900, 227 ff.

The words must be carefully selected. The following are the results of an experiment. The visual-verbal image in Series i., and the auditory-verbal image in Series ii., are not recorded.

Series I. — Word Seen		Series II. — Word Heard	
Driftwood	V, V	*Summer night*	V, V-V
Waves	V, T	*Picnic*	V, V-V
Violet	V, V	*Fog horn*	V, A
Brook	A, V	*Landscape*	V
Salt	G, V	*Fire*	V

Series I. — Word Seen		Series II. — Word Heard	
Quail	A, V, V	*Blackbirds*	V, V
Sea shell	V, V	*The morning wind*	P, A, O
Waterfall	V	*Glowing horseshoe*	V, V
Rose	V	*Flute*	A
Wet sidewalk	V, M	*Ice water*	V, T
Railroad	A, O, V	*Brass band*	A, V, V
Springtime	V, V, A	*Horse*	V
Infinity	V, V, V, O	*Bees*	V
Fog	V, V, P, T	*Robin*	V
Dog	V	*Chimes*	A, V, O
Leaves	A, V, O	*Kerosene*	V
Expanse	V	*Gas*	V, V, O
Bright day	V, V, O	*Slate roof*	V
Brimstone	V, O	*Steam whistle*	A, V, O
Forest	V, O, A	*Inkstand*	V

Here V = visual; A = auditory; P = pressure; O = organic; T = temperature; G = gustatory; M = motor; V–V = visual-verbal.

If we sum up the results, giving an unit to every word, and counting all the ideas of a single category as one (so that *Driftwood* gives 'one visual'; *Bright day* gives 'one-half visual, one-half organic,' etc.), we obtain:

Visual Series			Auditory Series		
V	12.8	64.0 %	V	14.6	73.0 %
A, O	2.5	12.5 "	A	2.9	14.5 "
T	.8	4.0 "	O	1.4	7.0 "
G, M	.5	2.5 "	T	.5	2.5 "
P	.3	1.5 "	P	.3	1.5 "

It is clear, even from so brief a series as this, that *O* is predominantly visual, and that auditory and organic 'images' follow the visual at a very long interval. It is noteworthy that there is not a single A–V image in Series i.

(3) *The Questionary or Galton's Method.*

"We may assert, without fear of contradiction," says Külpe (Outlines, 185), "that the number of discriminable qualities of centrally excited sensations in general is less than that of the peripherally excited qualities." The author is not convinced by Külpe's reasoning, and hopes that the question may some day be put to the test of experiment. In the meantime, it has been suggested to him by Dr. Bentley that questions which require an accurate discrimination between similar images might be introduced, with good result, into a questionary upon ideational type. The following are typical distinctions:

(*a*) The clangs of locomotive, door, school, church and dinner bells. The roar of wind, waterfall, distant thunder, distant cannon.

(*b*) The colours pink, carmine, blood red, rose. The different patterns of wall paper in a well-known house.

(*c*) The tastes of apple, pear, quince (with full analysis).

(*d*) The 'feels' of silk, satin, velvet, plush, as the finger is passed over them.

(*e*) The scent of geranium, rose wood, cedar wood, sandal wood.

(*f*) The organic complexes in weak, strong, slow and quick anger.

(4) *The Method of Letter Squares or Binet's Method.* — This method has been modified and extended, in the Cornell laboratory, as follows.

EXPERIMENT, PART I. *Without Direction.* — Materials: 4 letter squares, 4 blanks; 4 series of 9 letters; 4 irregular figures. [The letter squares are cards or papers, 10 by 7.5 cm., divided into 12 squares (2.5 × 2.5 cm.), each of which contains a letter. The letters are consonants, printed in gothic type, about 8 mm. in height. They are arranged in random order; but any collocation that might serve as an aid to memory should be studiously avoided. The blanks are similar cards, ruled in squares without the letters. The letters in the 9-letter series are also arranged at haphazard. The irregular figures are simple 'nonsense diagrams,' made up of 6 curved or straight lines, within an area of about 20 by 15 cm.]

(*a*) *O* sits at a table, upon which are a blank square and a letter square turned face downwards. At the "Now!" he turns over the letter square, and learns the letters. After 10 sec., at another "Now!" he lays down the letter square, and counts aloud 1 to 20 for 20 sec. At a third signal, he proceeds to fill out the blank square from memory. Ten sec. are allowed for the writing. Then, at a fourth "Now!" *O* writes on the back of the blank square an account of his method of reproduction.

Illustration: letter square given:	Q	H	B	K	reproduced:	Q	H	R	T
	Y	N	P	G		Y	N		V
	F	T	C	V		F	C		G

Introspection: I visualised the card, and the third column was blank. Then I said over the letters by groups of three. The second group suggested R for the third column, but I could not see it on my visualised card.

The test is to be repeated with the remaining three cards. The results may then be evaluated, on an arbitrary scale, as follows. All letters given in introspection as reproduced by a pure memory (V; A; M) are to count as 1; all letters given as re-

produced by double memories (V + A; V + M; A + M) are to count ½ to each partial memory concerned; and all letters reproduced by mixed memory (V + A + M) are to count ⅓ to each partial memory. Under these rubrics, a rightly placed and rightly remembered letter counts as 1; a rightly remembered but misplaced letter counts as ½; a substituted letter counts as ¼; and an omitted letter counts, of course, as 0.

Illustration: square given:	B K F P	reproduced: B K F G
	D X T M	D X T M
	R L J S	R H N

First two horizontal lines V; last line A + M.

Visual: correct, 7; misplaced, 0; substituted, 1 (value ¼); omitted, 0. Total, $\frac{29}{48}$.

Auditory-motor: correct, 1; misplaced, 0; substituted 2 (value ½); omitted, 1. Total, auditory, $\frac{3}{48}$; motor, $\frac{3}{48}$.

General result: — ratio V : A : M = 29 : 3 : 3. Total memory = $\frac{35}{48}$.

(*b*) *E* takes one of the 9-letter series, and reads it to *O*. He must read evenly, clearly and slowly, — giving at the quickest not more than 2 letters in the 1 sec., — with entire avoidance of rhythm. At the end of the series, *O* recalls the letters, and dictates them, in what he thinks their right order, to *E*. He then describes his method of reproduction.

The test is to be repeated with the remaining three series. The results are worked over as before.

(*c*) *O* closes his eyes, and takes a pencil in his hand. *E* lays one of the drawings upon the table, and guides *O*'s hand along its outlines. The movement must be slow and continuous, and the pencil in *O*'s hand must never leave the paper. When the tracing is complete, *E* gives *O* a blank sheet of paper, and *O* reproduces the drawing with eyes closed. He then describes his method of reproduction.

E estimates the accuracy of the drawing on the basis of 60 (10 units for each of the 6 lines). The test is repeated with the remaining three drawings.

It is clear that, although no directions as to mode of reproduction are here given, the first test encourages a visual, the second an auditory-motor, and the third a visual-motor memory. Rough as the method of calculation is, it is alike for all *O*'s, and will furnish a fair statement of the relative availability of

the different images. The statement is checked and refined by the results of the following experiment.

Note that the above 12 tests are not to be taken in regular sequence (4 squares, 4 series, 4 drawings), but to be intermixed in random order. Note also that the method of evaluation yields a structural and not a functional result. In the second illustration under (*a*), *e.g.*, the A + M memory, which functions singly, is split up into an A and an M.

EXPERIMENT, PART II. *With Direction.* — Materials: 6 letter squares, 6 letter series, 4 drawings.

(*a*) *O* learns a letter square by vision alone (or with emphasis on vision). He reproduces by writing from visual images. Two squares.

O learns as before, but reproduces by writing from auditory images. Two squares.

O learns as before, but reproduces by writing (with eyes closed) from motor (articulatory or finger-movement) images. Two squares.

(*b*) *O* learns a letter series by hearing alone (or with emphasis on hearing). He reproduces by dictating from auditory images. Two series.

O learns as before, but reproduces by writing from visual images. Two series.

O learns as before, but reproduces by writing (with eyes closed) from motor (articulatory or finger-movement) images. Two series.

(*c*) *O* learns an outline by movement alone (or with emphasis on movement). He reproduces by drawing from movement images. Two drawings.

O learns as before, but reproduces by drawing from visual images. Two drawings.

The results are worked out as before, and combined with those of the preceding tests to form a total picture of *O*'s mental furniture.

Like all work in 'individual' psychology, this experiment demands of *E* a certain tact and interpretatory insight. One may say to a psychologist "Reproduce by articulatory images," and he will understand the requirement; but with novices one must use a more concrete and suggestive formula. The following list of phrases, taken from the introspective records of a number of students, may be of assistance to the Instructor.

(*a*) *Pure Visual Memory.* — Visualised C. Every letter was seen. B seemed blacker than the rest. I saw the whole card. Visual image. The

letters arranged themselves in groups of two; the image of the second was always fainter than the picture of the first.

(*b*) *Pure Auditory Memory.* — The R seemed to ring through my head. Letters came by sound; I did not speak them. I remembered L because of its sound, which I like.

(*c*) *Pure Motor Memory.* — Remembered by the feel of the muscle. Wrote in a mechanical way; nothing there but the writing; seemed reflex. Tried to remember the way the muscles of my hand felt when I traced on the paper. The long upward stretch gave a sensation in the fore-arm, and the motor memory was quite easy.

(*d*) *Visual-motor Memory.* — Looked over the card: remembered G because it started the group, and A because it was diagonally across. Drew from a visual picture of mountain range and bridge. Movement suggested a moccasin flower, which I visualised. Thought the curves must be like the lines of a capital M. I said the first two letters with my lips as I read them, and afterwards in writing moved my lips.

(*e*) *Auditory-motor Memory.* — Heard the series as I wrote it; my throat and lips moved at certain letters. Remembered after saying over the first line in a sort of rhythm. Letters went in a rhythm, in groups of four.

(*f*) *Mixed Memory.* — Everything seemed to come in; the memory was certainly not a pure type, but very mixed. I could not make the rhythm of the third line go right until I saw the card. Saw and heard the letters: remembered that the last letters were formed by the lips. With the upward movement seemed to hear the word northeast; thought of a compass, and visualised it in its place in a ship.

(*g*) *Associations.* — Remembered R and S because they go together. Thought of algebra when I heard X. Letters of first line made donkey (D, N, K, J). Remembered that the last column was all letters towards the end of the alphabet.

See A. Binet and V. Henri, L'Année psychologique, ii., 1895, 442; É. Toulouse, Enquête médico-psychologique, etc. (Émile Zola), 1896, 182; S. E. Sharp, Amer. Journ. of Psych., x., 1899, 353, 370.

(5) *The Method of Letter Squares or Cohn's Method.* — This method enables us to compare the relative values of the visual and the auditory-motor memory in a given individual.

MATERIALS. — Letter squares and blanks.

EXPERIMENT. — (i) At the word of command, *O* turns the letter square, and reads the consonants aloud, twice over. He then replaces the square, and counts aloud 1 to 20, as before, for 10 sec. At the second word of command, he ceases to count, and proceeds to enter the letters that he recalls upon the blank square.

(ii) At the word of command, *O* turns the letter square, sounds the vowel *a* ('Ah!') continuously, and reads the letters through by vision, twice over. He then replaces the square, begins to count aloud 1 to 20, and continues the counting for 10 sec. At the second word of command, he ceases to count, and enters the letters that he recalls upon a blank square.

It is clear that the first procedure favours the auditory-motor, the second the visual memory. The results may be worked over as before.

For further methods, and for a more elaborate mode of evaluating the results, see J. Cohn, Zeits. f. Psych., xv., 1897, 161. *Cf.* also H. Münsterberg, Psych. Rev., i., 1894, 34.

(6) *The Method of Distraction or Washburn's Method.* — This method requires two *E*'s. The one *E* reads aloud to *O* some interesting tale; the other names numbers, which *O* is to add together.

O attends as completely as possible to the reading, and (in order that *E* may have a check upon the degree of attention) is required, at the end of each experiment, to write out a *résumé* of what he has heard. The adding is continuous: *i.e.*, each number given by *E* is added by *O* to the total already reached. *O* states the result of every separate addition, and the figures are recorded by *E*. The experiment may last from 5 to 10 min., according to *O*'s powers of endurance.

At the end of the experiment, *E* has (*a*) *O*'s *résumé* of the tale, which shows the direction of his attention and so indicates the success or failure of the method; and (*b*) the column of figures employed in the addition, together with *O*'s summations. If the experiment has been successfully conducted, *E* works over these numerical results, noting *O*'s mistakes as they occur. The character of the mistakes is an indication of *O*'s ideational type.

An illustration will make this clear. Suppose that *O* says:

312 and 15 = 317,
724 " 33 = 657,
599 " 29 = 618,

and so on: making mistakes of 10 and 100, while the number given for the digits-column is correct. We may be sure, without the introspective corrobo-

ration which we always obtain, that these mistakes are mistakes of vision. Suppose, on the other hand, that *O* says:

282 and 20 = 303,
569 " 23 = 593,
634 " 9 = 619,
668 " 14 = 674,

and so on: making mistakes which can hardly be explained from vision, but which suggest a sound-echo of one or other of the numbers summed. We may be sure, especially if we obtain introspective corroboration, — which the author has never failed to do, — that these mistakes are auditory or auditory-motor.

See Titchener, Mind, N. S., v., 1896, 238. For hints of a similar method, *cf.* Lay, 5; C. Féré, Rev. philos., xxi., 1886, 547.

(7) *The Method of Style or Fraser's Method.* — The thought-stuff of a writer may be inferred with a high degree of certainty from his writings. *Cf.* A. Fraser, Amer. Journ. of Psych., iv., 1891, 230; Lay, 24. A variant of the method (observation of the imagery aroused in one's own mind by the reading of a given author) is suggested by Lay, 29.

(8) *Miscellaneous Tests.* — (*a*) Image a red cross, and then gaze at a sheet of white paper. If the after-image appears, you are of the visual type. Binet, Psych. of Reasoning, 41; but *cf.* James, ii., 67 f. The author has never found a student who obtained the after-image, nor has he ever obtained it himself. See, however, J. E. Downey, Psych. Rev., viii., 1901, 42. (*b*) Let *O* learn a square of 25 figures or letters. If he is visual, he can repeat the figures in any order, — by diagonals, spiral-fashion, etc., — almost as easily as he can repeat them line by line; if he is auditory or auditory-motor, he will stumble and hesitate when called upon to travel out of the order of learning. Binet, Psych. des grands calculateurs, etc., 144, 146. (*c*) Stern (54) suggests an enquiry into the part played by the different senses in spatial perception, beginning with an exact study of the auditory and tactual types of the blind consciousness (T. Heller, Philos. Studien, xi., 1895, 109 f.). (*d*) An experiment of Meumann's (Philos. Studien, xii., 1896, 169) is made by Stern (54 ff.) the basis of a distinction between a formal and a material type of temporal perception.

Question (3) — See pp. 215 f., above; Stern, *op. cit.*; H. Münsterberg, as quoted p. 411, below; A. Binet, L'Année psych., iii., 1897, 315; S. E. Sharp, Amer. Journ. of Psych., x., 1899, 372 f.; G. E. Müller and F. Schumann, Zeits. f. Psych. vi., 1893, 265; etc., etc. *Cf.* also the doctrine of affective temperaments.

EXPERIMENT XXXVII

§ 58. **The Association of Ideas.** — The phrase 'association of ideas' is one of the most familiar and one of the most slippery phrases that are found, as a matter of course, in works upon psychology. It is a phrase with a long psychological history; and it has, naturally, in the course of its history, taken on many different shades of meaning. The facts that it covers are of high importance. The traditional English psychology — often spoken of as the psychology of 'associationism' — has not hesitated, time and again, to compare the operation of the law of association in the sphere of mind with that of the law of gravitation in the physical universe. And Wundt, who is assuredly not an 'associationist,' is emphatic upon the point that without association there can be no consciousness (Phys. Psych., ii., 1893, 256, 475).

What is meant by the phrase 'association of ideas'? It is sufficient for the student, at this stage, to distinguish five uses of the word 'association.' (*a*) Association is the technical term for all forms of mental (as contradistinguished from physical) connection. (*b*) Association is not simply a descriptive name for mental connection, but is the universal principle or 'explanation' of such connection. Over against these wider interpretations stand the following. (*c*) Association is the general name for all those modes of conscious connection in which the elements are still recognisable, as parts, in the compound. This use is hardly to be distinguished, in the older literature, from (*a*). In the days when ideas were hypostatised as mental atoms, and psychology moved almost exclusively in the intellectualistic world of visual and auditory-verbal elements, it was difficult to conceive of a mode of connection in which (as in the fusion) the parts should be merged in an unitary complex. (*d*) Association

is the explanatory principle of 'reproduction,' of the calling-up of a past experience by a present stimulus. Finally, (*e*) just as fusion may mean either a mode of intimate connection or its product, the weld or fused mass of elementary processes, so may association mean — not the mode of looser connection, or its explanatory principle, but — the resultant complex, the 'associated' itself.

We may, ourselves, at once reject the uses (*b*) and (*d*). Experimental psychology cannot employ 'association' as a principle of explanation. We may also reject (*a*): we do not speak of the formation of auditory rhythm, or of the tonal fusion, as a matter of association. There remain (*c*) and (*e*). And we must say here precisely what we said in the case of fusion (p. 330 above). The relation of associatedness is not something superadded upon the associated sensations. Two sensations, given together in temporal succession or in spatial contiguity, are given in the relation of association; the association is the look or sound or feel of the sensations as they occur. Nor must any process-meaning be read into the word 'association.' There is no trace of associating, of being associated, when the sense-processes appear. They form a group or a series; and this group or series, the sense-whole, is the association. We may abstract, in our logical thinking, the relation from the contents, and speak of association as "an observable connection between contents of consciousness" (Calkins; *cf.* p. 109 of the text); but there is no relation-process present, over and above the processes related. We are able, having taken up this attitude to association, to define the problem which it offers to experimental psychology. We have to enquire (1) under what conditions the associated complex makes its appearance in consciousness, and (2) what are the distinguishing characteristics of the complex, as compared with other mental formations. The first of these questions, as has been pointed out in the text, falls into a number of part-problems. All connections tend to persist: under what special conditions, then, is the given connection realised? The question can be answered, with some degree of completeness. For an answer to the second enquiry, on the other hand, we are thrown back upon descriptive psychology.

It is a little curious, at first thought, that the association of ideas—apart from the question of its time relations—should have been handled in stepmotherly fashion by experimental psychology. The riddle is, however, easily read when we remember the historical conditions under which experimental psychology arose, and note the coldness with which the experimental method has been received, on its side, by English psychologists. The new psychology came, in Germany, by way of revolt against the metaphysical psychology which reigned before Herbart, and which Herbart himself, standing on the line of division, both accepted and rejected; it came, in large measure, from the hands of men who had received their training in natural science; it was to be a scientific psychology. But England had possessed, ever since the time of Hobbes, an empirical (if not a scientific) psychology. There was no need of revolt. If the traditional psychology was, in reality, rather a theory of knowledge than a science of mind, the result was only that it seemed to be so much the more practical, so much nearer to the actual use and employment of mind. Its 'ideas' were the current coin of human intercourse, meanings, 'universals'; but they passed for psychological facts. Imagine a psychologist of this school. What could he expect of the experimental method, that should be better than the masterly developments of the associationist doctrine to be found in the pages of Bain and Spencer?

Consider, on the other hand, the German experimentalist, with the spirit of revolt keen within him. Consider his philosophical ancestry, and his power to draw just those distinctions which the English psychologists did not draw. He might heartily admire the skill and patience of the associationist writers, but he could hardly sympathise with their position. Moreover, if he wanted a *Vorstellungsmechanik*, was there not Herbart?—and the differences between Herbart and the English school would seem greater than we now know them to be. So it is but natural that association, except in so far as it is open to measurement, has been neglected. And it is significant that the first investigation made, even into the time relations of association, was made by an English psychophysicist (Galton, Brain, ii., 1879, 149); and that the two monographs which deal with association for its own intrinsic sake, those of Scripture and Calkins, are written by English-speaking psychologists.

On Association in general, see the art. by G. C. Robertson, in the Encyc. Britannica, 9th edition; James, Principles, i., 550; Wundt, Phys. Psych., ii., 437; A. Bain, The Senses and the Intellect, ed. of 1868, 321; H. Spencer, The Principles of Psychology, i., ed. of 1881, 250; J. Sully, The Human Mind, ii., 1892, 339 (and references); G. T. Ladd, Psychology, Descriptive and Explanatory, 1894, 263; A. Allin, Ueber d. Grundprincip d. Association, Berlin, 1895. Külpe's chapter on Centrally Excited Sensations (Outlines, Pt. i., ch. iv., 169) is the most authoritative statement yet made from the experimental side, and will presently bear rich experimental fruit.

The law of association, as stated in the text, is that all the connections between sensations which are set up by the forma-

tion of perceptions and ideas tend to persist, even when the original conditions of connection are no longer fulfilled. This law makes the non-fulfilment of original conditions an extreme or limiting case. The name 'association' has, however, been narrowed down by historical usage to those connections, and to those connections only, which are realised in the absence of the original conditions of connection. And this restriction of meaning, if conventional, is also convenient; for it enables us to mark off the association from other conscious complexes. The primary connections, whereby sensations are grouped into perceptions and ideas, do not fall under the rubric of 'associative' connections. And, as the association appears when the original conditions of connection are not fulfilled, the second term of the association (second in point of time, or secondary in point of formation) must always be a centrally excited sensation. See the author's Outline, 201 ff.

MATERIALS. — The apparatus described in the text is the original form of Jastrow's Memory Apparatus, as sold for $12.00 by the Garden City Model Works, 124 Clark Street, Chicago, Ill. The instrument may be made of any size, as required; it can now be ordered from the Chicago Laboratory Supply and Scale Co. The author would advise that the openings in the horizontal strip of tin be made 6 and 3 cm. in length, respectively, and that they be separated by an interval of not more than 1 cm. It is well also to run a string from the hinged flap over a pulley placed at the top of the screen, in order that *E*, as he sits behind the instrument, may be able to open and close the window at will.

STIMULUS CARDS. — The cards for use in the *successive* method are prepared as follows. A piece of white cardboard is cut to fit the card holder. At the bottom of the card — the end that is to appear first behind the window — is pasted a strip of coloured paper, large enough to fill the larger opening in the middle of the horizontal strip of tin. Above this, at the right height, are pasted two (or more) black numerals. Then follows a coloured paper; then more white card and black numerals; and so on. As the card is dropped by the lever, *O* will see, first, a colour; then, a number; then colour again; and so forth,

for 14 exposures. Seven colours and seven numbers are thus seen in alternate series.

The cards for use in the *simultaneous* method are prepared in just the same way, except that colour and number are placed upon the same line. Fourteen colours and fourteen numbers can thus be shown, paired, in a single series. Twelve only are employed in the experiments.

Besides these, *E* must have test cards: cards which carry the colours of the various stimulus cards, but have no numerals. The colours are rearranged as the requirements of the experiment suggest. See examples, below.

The coloured papers may be obtained from the Milton Bradley Co., Springfield, Mass. Gummed numerals and letters are sold by the Dennison Mfg. Co., 198 Broadway, N.Y. City.

If the two openings are made of the dimensions recommended above, the stimulus card and test card of the successive method can be combined. The stimulus colours and numbers are pasted on the left of the white card, and the test colours on the right. The larger opening is then set at the left end of the oblong window, and the smaller at the right; the openings are blocked, as required, by extra strips of black japanned tin. *E* is thus spared the trouble of removing the card-holder for the exchange of cards. *O*'s fixation must be secured by a white paint-dot upon the closed shutter.

It is possible, in the same way — though it is less convenient — to employ three openings for the combined cards of the simultaneous method.

EXPERIMENTS (1)-(4). — The method of the first four experiments is that suggested by M. W. Calkins, Psych. Rev. Monograph Suppl. 2, 1896, 37 ff. Two principal modifications have been made. Calkins' exposure times and intervals have proved, in the author's experience, to be too long; and it is necessary to fill the intervals with some occupation (such as the repeating of the alphabet), and not to let *O* stare blankly at the shutter. If the interval is not filled in some way, *O* is likely to memorise the foregoing connection of colour with number during the 4 sec., and the whole series may be rightly repeated.

The following is a specimen Frequency series.

STIMULUS CARD	TEST CARD
Green, 47	Blue
Violet, 61 (*f*)	Brown
Brown, 73	*Violet* (*f*)

STIMULUS CARD	TEST CARD
Violet, 61 (*f*)	Green
Orange, 84	Orange
Blue, 12	
Violet, 25 (*n*)	

Calkins, 38.

Recency

STIMULUS CARD	TEST CARD
Peacock, 46	Grey
Blue, 38 (*n*)	*Blue* (*r*)
Brown, 51	Peacock
Strawberry, 85	Yellow
Grey, 74	Strawberry
Yellow, 29	Brown
Blue, 52 (*r*)	

Calkins, 39.

Vividness

STIMULUS CARD	TEST CARD
Brown, 34	Blue
Orange, 51	Dark red
Green, 792 (*v*)[1]	Violet
Blue, 19	*Green* (*v*)
Violet, 48	Brown
Green, 69 (*n*)	Orange
Dark red, 54	

Calkins, 38.

[1] [illegible] than the three-place number is a two-place number printed in unusu[illegible]-faced or thin-faced type, spaced widely, printed in colour, larger or smaller than the average, etc.

Primacy

STIMULUS CARD	TEST CARD
Light red, 48 (*p*)	Grey
Violet, 60	Yellow
Grey, 82	*Light red* (*p*)
Orange, 29	Blue
Light red, 31 (*n*)	Orange
Yellow, 53	Violet
Blue, 69	

Calkins, 39.

In these series, *f* = frequent; *r* = recent; *v* = vivid; *p* = prime; *n* = normal. The structure of the series will be evident on inspection. In every case, the 'preferred' numeral — frequent, recent, etc. — has two chances of association: the one a 'normal' chance, equal to that of the other terms of

the series; the other a weighted chance, greater than that of the other terms by the handicap which frequency, etc., bestow. See the evaluation of results, below.

The series are somewhat short, and it would be well, if the apparatus is made to order, to secure a card-holder arranged for 24 exposures.

If time does not permit of the taking of 80 series from every *O*, the number may be reduced to 40 or 20, and the results from the whole class thrown together for calculation. This is Calkins' procedure (*e.g.*, p. 42). *Cf.* the questionary method, and Stumpf's mass-experiments upon degree of tonal fusion.

Exps. (1)–(4) should not be performed in regular sequence, but carried on, so to speak, all at the same time The series can be sorted out, to their respective experimen · the work proceeds; so that there is no danger of confusio

RESULTS. — The following specimen Tables n from Calkins: the data which the author has at his extensive, though they offer a general confirmat percentages.

(1) General Table of Correct Associations.

NUMBER OF SERIES.	POSSIBLE CORRECT ASSOCIATIONS.	ACTUAL CORREC
		Full.
7-term 444	2144	674
10 to 12-term 867	7672	1728

(2) Specimen Special Table: Frequency 3:

NUMBER OF SERIES.	BOTH NUMBERS ASSOCIATED:			NORMAL ONLY ASSOCIATED:		
	Full.	Half.	%	Full.	Half.	%
200	37	3	19.2	7	9	5.7

It follows that the *f*-number is associated [illegible]3.7 % (44.5 ÷ 19.2 %) of the possible cases; the *n*-number in only 24.9 % (5.7

+ 19.2 %). These two percentages are to be compared with the 26.1 % of the foregoing Table.

Calkins' percentages for vividness are 52.2 and 20.8; for recency (short series), 53.7 and 25.7; for primacy, 36.5 and 29.5 (great individual differences). In the short series in general, the first number (primacy) showed an association percentage of 43 %.

Question (1) This Question may be answered exactly by the percentages of correct associations obtained in comparable series. Only, *E* must be sure that the series *are* comparable (*cf.* Calkins, 41). It may be said, in general terms, that frequency tands highest in order of efficacy;[1] then follow vividnes recency, with vividness in all probability slightly ahead primacy has the lowest position.

(2) be remembered that all four experiments presup- tive consciousness. We can hardly think that repetition, as a merely mechanical matter, — *i.e.*, state of attention, — would produce the effects l. Let the mature reader attempt to recall the his childish bedroom, to name the faces in a school tc. He will find that, despite the extreme fre- lkin stimulus, memory is somewhat surprisingly blank; se, vividness or interest come to its assistance ines, 211). It must be remembered, too, that smai ndition of exceedingly wide range, and that the nts which are at our disposal for purposes of ot pretend to do it even relative justice (Calkins, he expectation of a series, — not of a single stim- e distribution and adjustment of attention that ch expectation, must tend to reduce the influence d recency.

her like considerations will suggest themselves to . What the experiments tell us is that frequency, ins and vividness are real conditions of association in the consciousness. So much is certain; farther it is hardly o go.

[1] *Cf.* the discus n of the law of 'the reproduction of the general,' p. 378, above.

(3) In answering this Question, *O* must be careful to distinguish the four conditions studied from the fifth condition,—(*e*) Pt. i., p. 201.

EXPERIMENTS (5)–(8). See Calkins, 46 f.

In view of the extreme similarity of result, it is hardly worth while to require a given *O* to perform both sets of experiments. The class may be divided, half taking the successive and half the simultaneous method. Comparison of results is, of course, only relative.

RELATED EXPERIMENTS. QUESTION (4).—Auditory series may be made up in various ways. (*a*) Calkins employs nonsense syllables and two-place numbers, both pronounced by *E*. Vividness is here best obtained by reading the number in an unusually loud tone: one may also use a one-place number, speak with slowness or hesitancy, speak in falsetto, etc. Pp. 47 ff. (*b*) The author has found it better to substitute harmonium (or any form of reed) clangs for the nonsense syllables. The clangs stand nearer to the colours of the visual experiments than do the syllables.

Both of these experiments imply the successive procedure. For simultaneous work, one may form mixed series, whether (*c*) of colours seen and clangs heard, or (*d*) of numbers seen and clangs heard.

Finally, (*e*) comparative visual series (Calkins, 51 ff.) may be taken. Thus a 12-term successive series may contain a thrice repeated *f*-number and a 3-place *v*-number in combination with the same colour; or a 7-term series may be constructed in which the last colour (*r*) had appeared once before with a 3-place *v*-number, or at the beginning of the series (*p*), or twice before with the same number (*f*).

Further: the alphabet-repetition between experiments may be replaced by some other occupation: listening to short anecdotes or news items (Calkins, 53), arithmetical exercises, translation, etc. And the interval between stimulus-card exposures and test-card exposures (or their auditory equivalents) may be varied within wide limits.

These related experiments may bring out individual differences,—associative habits, preferences in the direction of atten-

tion, liability to fatigue or distraction, — but will hardly throw new light upon the conditions of association at large. And even in the sphere of individual psychology, we must not expect too rich a harvest (Stern, Psych. d. indiv. Differenzen, 69). The experiments present two main advantages. In the first place, they serve, by contrast, to emphasise the extreme delicacy, instability, complexity, transiency of the most substantive of all consciousnesses. And, secondly, they enable *O* to realise the very great difficulty of introspection in the associative field.

B. The condition which we are to examine is the relation of the given impression to the present contents of consciousness (p. 207, above). The method was suggested by Münsterberg (Beitr. z. experiment. Psych., iv., 1892, 17 ff.), and has been employed by W. B. Pillsbury (Amer. Journ. of Psych., viii., 1897, 355) and (independently) by Goldscheider and Müller, *op. cit.*, 156 ff. It rests upon the fact that, if a word is displayed for a brief time which presents some slight difference from another word, it is read as if this difference were not visible, provided that words have previously been pronounced to *O* which stand in intimate association to the other, slightly different word, but have nothing to do with the actual impression.

Münsterberg obtained his first hint of the method from R. Avenarius' Kritik d. reinen Erfahrung, ii., 1890, 472. The author is glad to avail himself of this opportunity to recommend the Kritik to psychological readers. Like certain writings of E. Mach and Richard Wagner, it belongs to a department of literature hardly to be termed psychological, and yet abundantly rich in subtle psychological observation.

Münsterberg's procedure differs from that of the text in that the 'wrong' words were given only occasionally, as puzzle experiments. See p. 21.

MATERIALS. — The object cards carry a monosyllabic word. This may be (*a*) a 'real' word, similar in form to the word which *E* means to suggest. Thus *part* will be read as *past*, if 'time, future' are pronounced; *fright* will be read as *fruit*, if 'pear, apple' are given. Or it may be (*b*) a mutilated word. The mutilation may be accomplished in three ways. We may omit a letter altogether. *Sige* will be read as *siege*, if 'fortress, war' are given. We may substitute one letter for another: *sixge*,

siage, etc. And we may blur a letter; say, by writing or pasting an *x* over the middle *e* of *siege*.

On methods of mutilation, and on the importance of the position of the omitted, etc., letter in the word, see Pillsbury, 355 ff.

The series of object cards may be made as complete and as long as the Instructor deems desirable. The point of the experiment — the misreading of the stimulus-word under the influence of association — can be demonstrated in a few minutes.

EXPERIMENT (9). — If *O* does not read the stimulus-word at the first exposure, he should keep his eye at the tube until its reading is possible. *E* notes the number of revolutions required. Most *O*'s withdraw their eye from the tube as soon as ever the 'suggestion' has operated. Should an *O* incline to await further exposures, in order to verify the suggestion, he must be cautioned to read the word at the earliest possible moment: otherwise — since the word falls within the range of attention — he will presently notice the mutilation, and so get an inkling of the nature of the experiment.

When *E* takes the place of *O*, he will, of course, be working 'with knowledge.' A comparison of the two sets of results is instructive. The number of misreadings is reduced, but by no means reduced to zero. The new *E* should introduce (and should inform *O* that he has introduced) a certain number of correctly printed cards into the series ('puzzle experiments').

On the time of exposure, and its regulation, see Pillsbury, 345. The essentials of Scripture's and Pillsbury's apparatus (criticised by J. Zeitler, Philos. Studien, xvi., 1900, 441) are given with the after-image apparatus of Exp IV. (see Pt. i., Fig. 5).

Question (5) The general conclusion is that a close relation of the new impression to the present contents of consciousness — one of the determinants of passive attention — is of high associative importance. We have not been able so to arrange the experiment that condition (*e*) can operate in pure form. To that end, we should have to take an *O* who was in a state of day-dreaming or reverie, to subject him to stimuli, some of which were and some of which were not related to his train of ideas,

and to show that the former did while the latter did not alter the direction of the train. The task is difficult, if not impossible; and our experiment serves every purpose.

(6) Mutilated sentences are spoken into a phonograph, and then repeated to *O*. See W. C. Bagley, Amer. Journ. of Psych., xii., 1900, 80 ff.

(7) This question has been answered dogmatically in the answer to (5) above. The student will be able to work out the reasons for that answer on his own behalf.

C. The Train of Ideas. — It is customary to employ printed or spoken words as the stimuli to the train of ideas. Words were chosen for this end by Galton, and have been much used by later writers. The advantages of the word-stimulus are obvious; and, under certain conditions of experimentation, we have hardly any choice but to accept it. On the other hand, the isolated word is not the natural starting-point of an associative series; the mental unit is the sentence. Hence the author recommends the present exercise as a substitute for the word-test. It seems, at first sight, to be somewhat complicated; but it has stood the test of laboratory practice.[1] The following illustration will speak for itself.

Question. — Who was king of England in the year 1654?

Report of Introspection. — Saw outline map of England and Scotland, the southern part more distinct. Saw the number 1654, and tried to remember who was king (verbal). Thought of Gurth and Wamba. Woods; dining-room in Saxon house: verbal and visual, but forest clearly visual. Knew that Scott (verbal) was the author of the book. Tried to think of name of book. Ivanhoe; verbal-auditory. Tournament; visual picture of Disinherited Knight. Date bothered again. Indistinct picture of C. U. football team on Percy Field. Line-up rather distinct. Lehigh and Princeton games (verbal-auditory). Verbal idea of own practice.

The introspective report may be thrown into tabular form as follows.

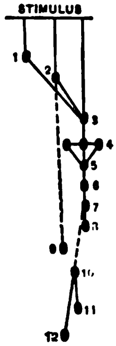
[1] It will be necessary to give some half-dozen preliminary tests, before the exp. is seriously undertaken. The associative consciousness is so complex, and its constituents so elusive, that the result of the first few trials will, in all probability, be nothing but confusion. A careful *O* will, however, very soon bring order out of the chaos.

Table

Total time, 160 sec.; catch words, 4.

	Situations.	Time.	Quality.	Affective Tone.	Mode of Connection.	Richness, etc.	Point of Departure
1	*Map*	2 sec.	Visual	Indifferent	Contiguity	All clear, but best in lower parts	Stimulus
2	Date		Visual and organic	Unpleasant	Contiguity	Clear	Stimulus
3	King		Verbal-auditory and visual; but chiefly organic	Unpleasant	Contiguity	Full, but vague	Stimulus, and 1 and 2
4	*Gurth* and Wamba, supplemented by woods, dining room	40 sec.	Visual, verbal-auditory, organic; the forest visual	Pleasant, so far as pictures were concerned; unpleasant, when I tried to remember author's name	Similarity, with associative supplementing	Clear, with vague fringe	Last idea
5	Scott		Verbal-auditory, organic	Pleasant	Contiguity	Clear	Last idea
6	Ivanhoe		Verbal-auditory	Pleasant	Contiguity	Clear	Last two situations
7	*Tournament*	75 sec.	Visual and verbal-auditory	Pleasant	Contiguity	Word clear: details of visual imagery indistinct: full consciousness	Last three situations
8	Knight		Visual	Pleasant	Contiguity	Consciousness 'thinner'; image more distinct	Preceding situations
9	Date		Visual and organic	Unpleasant	Similarity ?	Clear	Stimulus, or its immediate associates
10	*Football*	125 sec.	Visual (and motor ?)	Pleasant	Similarity	Indistinct in detail; line-up clear	Situation 7
11	Lehigh and Princeton games		Verbal-auditory	Indifferent	Contiguity	Indistinct	Last idea
12	Practice		Verbal-auditory (calls)	Indifferent	Contiguity	Very indistinct	Situation 10
		Exp. ended at 160 sec.					

The diagram, which should be paralleled by the time-scale, will have the following form.



It may, of course, be made very much more elaborate. The situations may be more fully analysed, in Scripture's way; different kinds of lines may represent the two different modes of connection; the 'fringes' and 'strands' of consciousness may be indicated, etc., etc.

It is clear that an experiment of this sort will throw some light upon the mechanism of the associative consciousness, will afford training in introspection, and will drive home the lessons taught by the Related Experiments of Question (4). The experiment has never failed, in the author's experience, to elicit such remarks as: "What a tangle of stuff there is there!" or "I never knew before what an idea was really like!"—whereas the association-series that follows upon a word-stimulus is apt to be as artificially clean-cut and over-focalised.

For word associations, and a classification based upon them, see T. Ziehen, Die Ideenassoziation d. Kindes, i., Berlin, 1898 (Sammlung v. Abh. aus d. Gebiete d. pädagogischen Psych. und Physiol., herausg. v. H. Schiller u. T. Ziehen, i., 6).

Question (8) This Question is answered in what follows. The Instructor may avail himself of it to take the student as far into the general psychology of association as time permits.

The experimental literature of association falls into two main divisions: the work done by the reaction method, and the monographs of Scripture (incomplete) and Calkins. Under the former heading fall the investigations of M. Trautscholdt (Philos. Studien, i., 1883, 213), J. McK. Cattell (*ibid.*, iv., 1888, 241), E. Kraepelin (Tagebl. d. Naturforschervers. z. Strassburg, 1885; Ueb. d. Beeinflussung einfachster psych. Vorgänge durch einige Arzneimittel, Jena, 1892), G. Aschaffenburg (Psychol. Arbeiten, i., 1895, 209), and H. Münsterberg (Beitr., i., 1889, 64). We have ourselves drawn for experimental material upon Münsterberg (Beitr., iv., 1892, 17) and Calkins. The first part of E. W. Scripture's research is to be found in the Philos. Studien, vii., 1892, 50. A parallel investigation by Münsterberg (mentioned in Beitr., iv., 24) has not as yet been published.

If we sift out the general results of the enquiries which have not been considered in what precedes, we seem to find the following laws. Several of them are generalisations from incidental remarks or single experimental results: all would repay reinvestigation. And even if all are valid, they make but a poor showing as against the complexity of the concrete consciousness, the "ununterbrochene Verflechtung, in welcher alle Dispositionen einmal gehabter und unserem Bewusstsein noch verfügbarer Vorstellungen mit einander stehen."

I. (1) Temporal connections of ideas stand, as regards quickness of realisation, in the order unequivocal, ambiguous, free. The proportion is, roughly, 3 or 4: 5 or 6: 7 tenths-of-a-second. The rule is a rule of average, and has many exceptions. (2) The more frequently an idea has been connected with other ideas, the more quickly and readily does it associate in the experimental case. (3) The more direct the temporal connection of two ideas, the more quickly is it realised. (4) The more closely related a given idea is to another idea, the more quickly does it connect with this other. (5) The more frequently a given form or order of connection between ideas has occurred, the more quickly is it realised in the experimental case. (6) The more intensive or clear the idea, the more quickly does it connect with other ideas.

II. (7) The more intensive or vivid idea connects with the more intensive or vivid idea. (8) The more frequently or permanently an idea has been present in consciousness, the more vivid and clear are the ideas that connect with it. (9) Related ideas frequently connect with the same idea. (10) Many peculiarities of association can be explained by reference to a law of exclusion. "When a simultaneous or successive connection of three contents, *a*, *b* and *c*, has established a liability of reproduction between *a* and *c*, *c* gradually comes to be directly excited by *a*, without the intermediation of *b*" (Külpe, Outlines, 209).

This law of exclusion suggests the doctrine of 'association by unconscious intermediaries,' maintained by Scripture (83), accepted in modified form by Wundt (Human and Animal Psych., 306 f., 3d German edn., 349 f.; Philos. Studien, vii., 360 f.; Phys. Psych., ii., 459 f.) and Aschaffenburg (Psychol. Arbeiten, i., 1895, 244, 294), but negatived by the work of Münsterberg (Beitr., iv., 1892, 1), H. C. Howe (Amer. Journ. of Psych., vi., 1894, 239) and W. G.

Smith (Mind, N.S., ii., 1894, 289; Zur Frage d. mittelb. Ass., Leipzig, 1894). *Cf.* also W. Jerusalem, Philos. Studien, x., 1894, 323, and Wundt, *ibid.*, 326. The author believes, with Münsterberg, that there is no such thing as an association by unconscious intermediaries. He is, however, of the opinion that associations occur in experimental practice which represent various stages or degrees of the habit-process which culminates in the law of exclusion.

The concept of psychological 'relationship' must be worked out by the student, as we worked out the concept of 'similarity' above, pp. 54 f.

Experiments upon the reaction time of association can be carried out by aid of the vernier chronoscope. See Sanford, Amer. Journ. of Psych., ix., 196 f.

The question of the classification of successive associations, like the question of the classification of the emotions, is an old one in psychology. We have, in the text, kept the rubrics 'association by similarity' and 'association by contiguity.' Wundt's substitution of the terms 'intrinsic' and 'extrinsic' association — the former dependent upon the principle of associative relationship, the latter upon that of associative practice — marks a distinct step in advance. But the advance consists rather in the explication of the principles than in the change of classificatory names. Wundt's own classification (Phys. Psych., ii., 455) is logical, not psychological, in its details. The still more elaborate classification of Aschaffenburg (*op. cit.*, 231), which is based upon Wundt's dichotomy, has little psychological value. On the other hand, Münsterberg's distinction of three intellectual temperaments, the subordinating, coördinating and superordinating (Beitr., iv., 36), although it is open to criticism on several counts, does good service in laboratory work.

In the author's opinion, — an opinion resting on several years' class-work in association reactions, — Münsterberg's three temperaments come nearer to the true psychological 'type' than is admitted either by Stern (*op. cit.*, 69) or by Aschaffenburg (225). Münsterberg himself grants (33) that the classification is not exhaustive.

The reader who is interested in the question of classification may consult further: F. Paulhan, L'activité mentale et les éléments de l'esprit, Paris, 1889; R. Wahle, Vierteljahrss. f. wiss. Philos., ix., 1885, 404; M. Offner, Philos. Monatshefte, xxviii., 1892, 385, 513; B. Bourdon, Rev. phil., xxxii., 1891, 561.

Wundt's Bemerkungen zur Associationslehre (Philos. Studien, vii., 1892, 329) represent the first attempt at a psychological theory of the association. The paper was prompted by the well-

known controversy between H. Höffding and A. Lehmann (see references, pp. 331 ff.); but its conclusion follows directly from Wundt's primary distinction of intrinsic and extrinsic associations. "All associations," says the summary in the Phys. Psych., ii., 468, "are the resultants of elementary connective processes[1] between simple sensations or relatively limited sense-complexes. Two such elementary processes are conceivable; and both may be traced in every instance of association. They are the connection of identical elements, and the connection of elements which have entered into a functional interrelation by their common occurrence in consciousness.[2] We will term these two forms of elementary connection the connection of identity and the connection of contiguity. These names suggest the customary terminology of association. But we do not mean to imply that what is usually called an 'association by similarity' can be analysed into elementary connections of identity, or an 'association by contiguity' into elementary connections of contiguity. Both alike depend upon the simultaneous operation of the two elementary processes." The formula of the former, roughly stated, is *abc-bcd*; the formula of the latter *abc-cde*. We have, then, the connections of identity *bc-bc* and *c-c*, and the connections of contiguity *a-d* and *ab-de*. It is clear that the term 'connection' in the phrase 'connection of identity' is only figurative; the qualitative contents of the original sensation or sense-complex remains unchanged; the sense-link changes merely in intensity and (more especially) in power over the attention. "We retain the term 'connection,'" says Wundt (469), "in order to emphasise the equal significance of the two—always coexistent—processes. . . . The relation of the identity to the contiguity connections may be expressed in the proposition that the former enhance the effect of a given ideational

[1] 'Processes' in the ordinary sense of 'occurrences or operations in time,' not in the technical sense in which we speak, *e.g.*, of sensation as a 'mental process.'

[2] In speaking of the identity connection, Wundt uses the terms *gleich, sich deckend, übereinstimmend,* [den beiden Vorstellungen] *gemein;* in speaking of the contiguity connection, the phrases *das zeitlich und räumlich durch Berührung Verbundene, die Bestandtheile die in früheren Vorstellungen mit jenen gleichen Elementen in äusserer Berührung gewesen waren, die Elemente die durch gemeinsames Vorkommen in einen functionellen Zusammenhang getreten sind.*

element upon consciousness on the intensive, the latter on the extensive side."

It is but natural that we should look for confirmation of this analytic theory to Scripture's monograph, the chief aim of which was "the collection of a large number of individual facts, from which conclusions might be drawn as to the particular conditions of the association of ideas" (51). And for one who reads between the lines, Scripture's article is rich in suggestion. Unfortunately, the author himself is trapped in the logical pitfall which has swallowed up so many of the association psychologists. His four categories of preparation, influence, apposition and after-effect are logical, not psychological, in character. This is shown partly by the writer's subdivisions (see, *e.g.*, 88), but still more plainly by the evidence of overlapping which his instances afford. Logically, the four stages may be distinguished; psychologically, they run into one another, cross one another, form total processes whose dissection is entirely artificial. It is, however, only fair to say that Scripture's theoretical conclusions have not yet been published (146).

It is, perhaps, worth while again to call the reader's attention to the disparity obtaining between the 'idea' of the traditional English associationism and the *Vorstellung* of experimental psychology. See, esp., Wundt, Philos. Studien, vii., 358 f.; x., 1894, 121 ff.; Münsterberg, Beitr., iv., 26.

The second part of the problem which association sets to experimental psychology is the problem of "the distinguishing characteristics of the complex, as compared with other mental formations" (p. 403). A good deal of work has been done, as we have seen, upon the duration of the association; and much has been done also, though we have made no mention of it in this Volume, upon the duration and time-relations of the simple sense-processes which underlie the association. The question of the *intensity* of the association as compared with the intensities of its elements—unlike that of the fusion (Stumpf, ii., 41, 423 ff.; Külpe, 283)—appears not to have been discussed. We must start out from Külpe's definition of the colligation (21, 277), as the mode or pattern of connection typical, in analytical psychology, both of temporal and spatial perceptions and ideas, and of the association of ideas, and ask, first of all, as to the intensity of a spatial connection of sensations. What do we mean by the intensity of brightness and colour in 'a picture,' a colligated whole of visual elements? We mean, probably, a middle intensity, higher than that of the shades and lower than that of the lights in the composition. What shall we say, again,

of the *quality* of such a colligation?—a question parallel to that of the pitch of a fusion. We can only say the obvious. If the colligated qualities are the same, then we have, in the case of spatial colligation, a single quality of greater extension; in the case of temporal colligation, a single quality of greater duration. If, on the other hand, the colligated qualities are different, then we have a looser unity, one that runs the risk of simultaneous and successive contrast. The spatial colligations of everyday life—our dress, furniture, house decorations generally—are evidently planned with implicit reference to this danger.

The questions of the intensity, quality and space-relations of the colligation offer an inviting field for new work.

FURTHER EXPERIMENTS.—For other ways of attacking the association problem, *cf.* the following.

(1) J. A. Bergström, Amer. Journn. of Psych., v., 1893, 356; vi., 1894, 432; H. Münsterberg, Beitr., iv., 1892, 69.

(2) H. Ebbinghaus, Das Gedächtniss, Leipzig, 1885; H. Münsterberg, Zeits. f. Psych., i., 1890, 99; G. E. Müller and F. Schumann, *ibid.*, vi., 1893, 81, 257 (and later articles from Müller's laboratory in the same journal); G. E. Müller u. A. Pilzecker, Experiment. Beitr. zur Lehre vom Gedächtniss, Leipzig, 1900 (critique of Calkins, 155 f.).

APPENDIX I

Examination Questions

The following questions are taken from examination papers set during the last few years in the qualitative part of this Course. They give a rough idea of the standard which the student may be expected to attain.

I

1. What are the chief phenomena of colour contrast? How are they explained (*a*) by Helmholtz and (*b*) by Hering?

2. Define the terms and phrases: local adaptation, disposition, simultaneous light induction, valence, rise and fall of sensation, flight of colours, after-image. What is the effect for vision of: the macular pigment, the visual purple, the imperfections of the dioptric media?

3. Mention some of the weak points of the Young-Helmholtz theory of visual sensations. State *briefly* the amendments proposed by Hering, von Kries, C. L. Franklin, Fick.

4. *Either:* Give Fechner's theory of the negative after-image. How would you set to work to test it, from Hering's standpoint?

Or: What apparent change does a red-green blue-yellow white-equation undergo with change of objective illumination? What explanations of it have been offered?

II

1. Describe the structure of the cochlea, including the arrangement of the terminal formations. Illustrate by diagrams.

2. What reasons led Helmholtz to modify his original theory of audition?

3. What are 'gaps' or 'tonal islands'? How are they to be explained by the Helmholtz-Hensen theory? Would this explanation be affected by the Ebbinghaus theory? How?

4. What are Rutherford's objections to the Helmholtz-Hensen theory? What is his own theory? What criticisms can be passed upon it?

5. What is the difference between 'structural' and 'functional' psychology? Illustrate by reference to the lectures or experimental work of the past term. Which is the more important to the beginner in psychology? Why?

6. What are the chief reasons for and against the existence of a third conscious element?

7. Explain clearly the relation of affection (1) to sensation and (2) to attention.

8. Can a psychological experiment be performed by the lecturer before a class? If so, is it advisable that experiments should be thus performed?

III

1. What are our reasons for supposing that the skin contains separate organs for pressure and for pain? And what are the reasons for the further assumption that the pain organs are situated more peripherally than the pressure organs?

2. What is Dessoir's classification of the provinces of Haptics? What criticisms have you to offer upon it?

3. Summarise Goldscheider's arguments in favour of the existence of a sensation of movement. Criticise them.

4. How would you set to work to discover the cold spots of the skin? Describe method, apparatus, etc. What special precautions would you take to avoid error?

5. Define experiment. How does a psychological experiment differ from the experiment of physical science? Should laboratory work in psychology be preceded by a lecture course? Why? Should a course of lectures in psychology be illustrated by demonstrations and desk-experiments? Why?

6. On the wall facing you hangs a spectrum chart. What facts of importance for the psychology of vision does an introspective examination of this chart bring out?

7. It is probable that we employ brightness (illumination of an object) as a criterion of distance; *i.e.*, that the brighter a thing is, the nearer (other things equal) do we take it to be. Suggest a simple form of apparatus for the investigation of this problem. Give drawings.

IV

1. What is the special method of psychology? How does it resemble, and how does it differ from, the methods employed by the physical or natural sciences?
2. What qualities of sensation occur in consciousness during the writing of a sentence upon paper?
3. Describe briefly the structure and mode of function of the auditory organ.
4. How would you set to work to discover what qualities of smell the nose can distinguish?
5. If with closed eyes you move the two hands outwards from the median plane of the body, and attempt to make your two movements of equal length, one hand will be found to travel farther than the other. Which hand makes the greater excursion, and why?
6. Analyse the perceptions of *resistance*, *impact*, *traction* and *wetness*.
7. What are the attributes of a 'perfect' sensation? Give instances of perfect and imperfect sensations.
8. Describe some recent experience which would throw light on:

    (*a*) your memory type;
    (*b*) your emotional temperament; and
    (*c*) your intellectual temperament or type.

V

1. Define 'local sign.' How has the system of local signs developed? Can you think of any alternative theory to that of local signature?
2. Give a full analysis of the perception of melody.

3. Distinguish 'perception,' 'idea,' 'simultaneous association of ideas.' What is the law of association? Apply it to instances chosen from each of the four main subdivisions of association.

4. Classify and explain the movements which 'express' emotion.

5. Outline the psychology of impulse.

6. *Either* (1) work out a classification of emotions, stating the principles upon which the classification is based; *or* (2) show, by references to examples, the importance of genetic psychology for the analysis of complex psychological processes.

VI

1. What do you mean by 'accommodation'? What is its organ, and how does that organ function?

2. What anomalous relations exist between stimulus and sensation in the sphere of sight?

3. State Wundt's theory of colour vision.

VII

1. State the facts of colour-blindness. To what facts of normal colour vision are they related, and in what way?

2. Describe the methods of colour mixture. What are the special advantages and disadvantages of the method with which you are yourself familiar?

3. Formulate the *general* dependence of visual sensation upon (*a*) amplitude, (*b*) frequency of oscillation, and (*c*) composition of the ether wave. Give a case of *special* dependence under each head, illustrating by reference to a concrete example.

4. Make a list of the general and special rules for introspection in experimentation upon simultaneous hand-movements ('innervation' sense). Discuss the question of the existence of an innervation sensation.

5. What are the constant sources of error to be guarded against in all psychological experimentation? Illustrate by an

analysis *either* of Aristotle's experiment *or* of the experiment on Weber's sensory circles.

6. What are the principal introspective data from which a theory of visual sensation has to set out? What are the principal anatomical and physiological data?

7. Give a theory of the intermittences observable in a sound that lies near the limit of audibility.

8. What are the chief reasons for and against the acceptance of the solar spectrum as the standard of reference in work upon colour vision?

9. Suppose that you were beginning an investigation into the pressure after-image. What problems would you set yourself for solution? Are they in any way parallel to the problems that arise in the investigation of visual sensation? By what methods would you work? Give full reasons for your choice.

VIII

1. What is 'introspection'? Give an instance of it.

2. What is the relation of *tone* to *noise*, physically and psychologically?

3. How many qualities of sensation are furnished by the skin? Give reasons for your answer.

4. What is the function of the internal ear?

5. Touch yourself on the back of the head.

   (*a*) What is the 'local sign' of this particular touch?

   (*b*) What processes may be involved in 'local signature' in general?

6. How would you set to work to show that different sensations proceed from skin, joint, muscle and tendon?

IX

1. Define and distinguish carefully between: affection, feeling, mood, emotion, passion, sentiment, temperament. Give an instance under each head.

2. How many qualities of affection are there? Support your answer by reference to introspective facts.

3. What is the importance of the reaction experiment? Describe the apparatus required for it, giving a diagram.

4. What is meant by 'individual' psychology? How is it related to psychology as a whole? Are there any other branches or sub-forms of psychology that are of special importance for general psychology?

5. Describe experiments for the determination of the extent and position of the blind spot. Why is the blind spot of importance psychologically?

6. How would you classify (1) emotions and (2) the expressions of emotion?

7. What theories have been suggested of the origin of the æsthetic sentiment?

8. Describe fully any set of experiments that you have made during the year, showing (1) the method employed, (2) the reasons for choice of method, (3) the character of the results and (4) the value of such results either for psychology as science or for your own training.

X

1. Distinguish between 'action' and 'movement.' What are the two current theories of the origin of voluntary movements? Who are their prominent representatives? Which do you prefer? Why?

2. What is the technique, and what the psychological value of the simple sensorial reaction? Illustrate.

3. Distinguish between idea and the simultaneous association of ideas. What is the function of the word-idea in the associative consciousness? Give examples.

4. Give a psychological analysis of the simple judgment. What is the place of judgment in a scheme of the intellectual processes?

5. What is the law of association? How does it differ from the laws of successive association formulated in the older psychology? What are these latter?

6. To what various analytic purposes can the reaction experiment be put?

XI

1. Give the laws of colour mixture.
2. Describe the refractive media of the eye.
3. How and why do you see the blood-vessels of the retina?
4. How are consonance and dissonance of tones explained?
5. Why do you see two images of the pin in Scheiner's experiment when the eye is not properly accommodated?

XII

1. What reasons have led to the assumption of
   (*a*) a sensation of innervation;
   (*b*) a sensation of muscular contraction?
2. State a theory of visual contrast.
3. Give some account of psychological method.
4. Discuss the following statements:
   (*a*) "Awareness of change is the condition on which our perception of time's flow depends." — James.
   (*b*) "A difference in the form of the stimulus, answering to a difference of quality in the sensation, affects the sense-feeling." — Sully.
   (*c*) "We cannot predicate Intensity of visual sensation." — Külpe.
   (*d*) "Preyer's theory refers the cognition of direction to the canals." — Münsterberg.

XIII

1. State and discuss some definitions of psychology.
2. How have sensations of smell and taste been investigated? What are the chief difficulties in their investigation?
3. What are the mental elements? How do you decide whether a process is elemental or not? What differences of opinion exist among psychologists in this matter?
4. In what sense may action be termed an association?
5. What is meant by 'complication experiments'? Describe them. Why are they important?

6. Give a theory of the feelings, naming its principal author or authors.

7. Explain the following terms and phrases:

(*a*) Sensation of difference.
(*b*) Original similarity.
(*c*) Recognition.
(*d*) Consciousness.

XIV

1. How does the method of psychology differ from the methods of the physical sciences?

2. Analyse the experiences of *hardness*, *smoothness*, *impact*, *resistance* and *sharpness* into their lowest conscious terms.

3. Give a theory of the pressure sense.

4. What are the functions of the semicircular canals of the internal ear?

5. State a method for the investigation of the articular sensitivity.

6. What is meant by the 'association of ideas'? What light does its study throw upon the constitution of mind?

7. What sensation arises when one looks at running water, over a precipice, etc.? Why?

8. Why should a cold weight seem to be heavier than a warm weight of the same objective heaviness?

XV

1. What problems does 'memory' present to the psychologist?

2. Discuss the possibility of an affective memory-type.

3. Explain: unity of consciousness, imagination mark, recept, aggregate idea, golden section.

4. What is the psychological basis of the æsthetic sentiments?

5. Define the place of the reaction experiment in a system of psychology.

6. Give the theory of psychophysical parallelism.

7. Wha. .wo types of action have been regarded as primitive? Which type does the psychologist advocate, and for what reasons?

8. Give a schema of the development of action. Give a concrete illustration of each type in the schema.

9. How would you find the time occupied by the purely associative processes in an association reaction? What suggestions can you make, as regards technique and computation in such an experiment?

XVI

1. Define: perception, state of consciousness, mental constitution.

2. Name the three forms of attention. Trace their growth, showing the conditions under which they have developed. What are the characteristics of the attentive consciousness? Do these characteristics differ in the three different forms of attention, or are they the same in all forms? What is the importance of the different forms of attention in the mental life of man?

3. Discuss the perception of rhythm, showing what psychological problems are involved.

APPENDIX I

BOOKS AND PERIODICALS

The student should be taught to connect the facts of experimental psychology with the names of their discoverers; so that the idea of 'temperature spots' shall be associated with the names of Blix, Donaldson and Goldscheider, — the '[illegible]' variation of [illegible] with that of [illegible], — the 'timbre' quality of tones with that of Helmholtz, etc., etc. Something should be known of the life and work of these investigators, and their original monographs should be read, or, at least, handled and glanced through, the plates studied, etc., wherever practicable. Even in an elementary course the student may be given information about the geographical distribution and historical development of the science that will prove of great value to him in later work.

The buying of books for oneself, the formation of a private library, should also be encouraged. If the student will spend even so small a sum as $1 a month he will have at the end of three years the nucleus of a working library.

For the school or college library, the following periodicals are essential:

(1) *L'Année psychologique.* Edited by A. Binet, with the collaboration of H. Beaunis and T. Ribot. 1895. Bibliography from 1894.
(2) *Philosophische Studien.* Edited by W. Wundt. 1881. Contains the work done in the Leipzig laboratory.
(3) *The American Journal of Psychology.* Edited by G. S. Hall, E. C. Sanford and E. B. Titchener. 1887.
(4) *The Psychological Review.* Edited by J. M. Baldwin and J. McK. Cattell. 1894. Bibliography (sold separately) from 1894.

(5) *Zeitschrift für Psychologie und Physiologie der Sinnesorgane.* Edited by H. Ebbinghaus and A. König. 1890. Bibliography from 1889.

Many of the volumes of these periodicals extend over a longer period than one year. Thus the first volume of the Philos. Studien is dated 1883, but the parts extend from 1881 to 1883. The author has sought, wherever possible, to date the articles cited in the text by the year of their part or number, rather than by that of their volume.

The following are the 50 books that, in the judgment of the author, will prove most useful to students taking this Course.

1. H. Aubert, Grundzüge der physiologischen Optik. Leipzig, W. Engelmann. 1876.
2. A. Bain, The Senses and the Intellect. 3d edn. London, Longmans, Green & Co. 1868.
3. A. Bain, The Emotions and the Will. 3d edn. London, Longmans, Green & Co. 1880.
4. J. M. Baldwin, Mental Development in the Child and the Race, Methods and Processes. 2d edn. New York, The Macmillan Co. 1899.
5. F. Brentano, Psychologie vom empirischen Standpunkte. Vol. i. Leipzig, Duncker & Humblot. 1874.
6. H. Ebbinghaus, Grundzüge der Psychologie. Erster Halbband. Leipzig, Veit & Comp. 1897.
7. G. T. Fechner, Elemente der Psychophysik. 2d (unchanged) edn. 2 vols. Leipzig, Breitkopf & Härtel. 1889.
8. M. von Frey, Ueber die Sinnesfunctionen der menschlichen Haut. i. Druckempfindung und Schmerz. Leipzig, S. Hirzel. 1896.
9. F. Galton, Inquiries into Human Faculty and its Development. London, Macmillan & Co. 1883.
10. A. Goldscheider, Gesammelte Abhandlungen. 2 vols. Leipzig, J. A. Barth. 1898.
11. H. L. F. von Helmholtz, Handbuch der physiologischen Optik. 2d edn. Hamburg and Leipzig, L. Voss. 1896.
12. H. L. F. von Helmholtz, On the Sensations of Tone as a Physiological Basis for the Theory of Music. Translated by A. J. Ellis. 3d edn. London and New York, Longmans, Green & Co. 1895.
13. V. Henri, Ueber die Raumwahrnehmungen des Tastsinnes, ein Beitrag zur experimentellen Psychologie. Berlin, Reuther & Reichard. 1898.
14. E. Hering, Beiträge zur Physiologie. Leipzig, W. Engelmann. 1861–1864.

15. E. Hering, Die Lehre vom binocularen Sehen. Leipzig, W. Engelmann. 1868.
16. E. Hering, Zur Lehre vom Lichtsinne. Wien, C. Gerold's Sohn. 1878.
17. E. Hering, Der Raumsinn und die Bewegungen des Auges. In L. Hermann's Handbuch der Physiologie, iii., 1, 343–601. Leipzig, F. C. W. Vogel. 1879. (This Part contains, also, A. Fick's Lehre von der Lichtempfindung.)
18. H. Höffding, Outlines of Psychology. Trans. by M. E. Lowndes. London and New York, Macmillan & Co. 1891. 2d German edn., 1893.
19. A. Höfler, Psychologie. Wien and Prag, F. Tempsky. 1897.
20. A. Höfler and S. Witasek, Psychologische Schulversuche, mit Angabe der Apparate. Leipzig, J. A. Barth. 1900.
21. W. James, The Principles of Psychology. 2 vols. London, Macmillan & Co.; New York, H. Holt & Co. 1890.
22. O. Külpe, Outlines of Psychology, based upon the Results of Experimental Investigation. Trans. by E. B. Titchener. London, Swan Sonnenschein & Co.; New York, Macmillan & Co. 1895.
23. G. T. Ladd, Psychology, Descriptive and Explanatory, a Treatise of the Phenomena, Laws and Development of Human Mental Life. New York, Charles Scribner's Sons. 1894.
24. O. Langendorff, Physiologische Graphik, ein Leitfaden der in der Physiologie gebräuchlichen Registrirmethoden. Leipzig and Wien, F. Deuticke. 1891.
25. A. Lehmann, Die Hauptgesetze des menschlichen Gefühlslebens, eine experimentelle und analytische Untersuchung über die Natur und das Auftreten der Gefühlszustände nebst einem Beitrage zu deren Systematik. Leipzig, O. R. Reisland. 1892.
26. A. Lehmann, Die körperlichen Aeusserungen psychischer Zustände. I. Plethysmographische Untersuchungen. Leipzig, O. R. Reisland. 1899. With atlas of 68 plates; Kopenhagen, 1898.
27. T. Lipps, Grundtatsachen des Seelenlebens. Bonn, M. Cohen u. Sohn. 1883.
28. T. Lipps, Raumaesthetik und geometrisch-optische Täuschungen. Leipzig, J. A. Barth. 1897.
29. R. H. Lotze, Medicinische Psychologie oder Physiologie der Seele. 1852. Reprinted, 1896. L. Horstmann, Göttingen.
30. E. Mach, Contributions to the Analysis of the Sensations [1886]. Trans. by C. M. Williams. Chicago, Open Court Publ. Co. 1897.
31. A. Mosso, Fear. Trans. by E. Lough and F. Kiesow. London, Longmans, Green & Co. 1896.
32. A. Münsterberg, Beiträge zur experimentellen Psychologie. Freiburg i. B., J. C. B. Mohr. 1889–1892.
33. T. Ribot, Psychologie de l'attention. Paris, F. Alcan. 1889. Trans., Open Court Publ. Co., Chicago, Ill. 1896.

34. E. C. Sanford, A Course in Experimental Psychology. Pt. i. Sensation and Perception. Boston, U. S. A., D. C. Heath & Co. 1898.
35. E. W. Scripture, The New Psychology. Cont. Sci. Series, xxxiii. London, Walter Scott, Ltd.; New York, Charles Scribner's Sons. 1897.
36. L. W. Stern, Ueber Psychologie der individuellen Differenzen. Ideen zu einer 'differentiellen Psychologie.' Leipzig, J. A. Barth. 1900.
37. G. F. Stout, Analytic Psychology. 2 vols. London, Swan Sonnenschein & Co.; New York, Macmillan & Co. 1896.
38. C. Stumpf, Tonpsychologie. 2 vols. Leipzig, S. Hirzel. 1883, 1890.
39. C. Stumpf, Ueber den psychologischen Ursprung der Raumvorstellung. Leipzig, S. Hirzel. 1873.
40. J. Sully, The Human Mind, a Text-book of Psychology. 2 vols. London, Longmans, Green & Co. 1892.
41. E. B. Titchener, An Outline of Psychology. 3d edn. London, Macmillan & Co.; New York, The Macmillan Co. 1899.
42. M. von Vintschgau, Physiologie des Geschmackssinns und des Geruchssinns. In L. Hermann's Handbuch der Physiologie, iii., 1, 145–286. Leipzig, F. C. W. Vogel. 1880. (This Part contains also V. Hensen's work on hearing, O. Funke's on touch and common sensation, and E. Hering's on the temperature sense.)
43. J. Ward, Psychology. Encycl. Brit., 9th edn., pt. 77. Edinburgh, A. & C. Black. 1886.
44. E. H. Weber, Der Tastsinn und das Gemeingefühl [1846]. Published as off-print from R. Wagner's Handwörterbuch der Physiologie, 1851.
45. W. Wundt, Grundzüge der physiologischen Psychologie. 4th edn., 2 vols. Leipzig, W. Engelmann. 1893.
46. W. Wundt, Lectures on Human and Animal Psychology. Trans. by J. E. Creighton and E. B. Titchener. 2d edn. London, Swan Sonnenschein & Co.; New York, The Macmillan Co. 1896. Third German edn., 1897.
47. W. Wundt, Outlines of Psychology. Trans. by C. H. Judd. 2d edn. Leipzig, W. Engelmann; London, Williams & Norgate; New York, G. E. Stechert. 1898. Third German edn., 1898.
48. W. Wundt, Die geometrisch-optischen Täuschungen. Leipzig, B. G. Teubner, 1898.
49. T. Ziehen, Introduction to Physiological Psychology. Trans. by C. C. van Liew and O. W. Beyer. 2d edn. London, Swan Sonnenschein & Co.; New York, The Macmillan Co. 1895. Fifth German edn., 1900.
50. A. Zwaardemaker, Die Physiologie des Geruchs. Leipzig, W. Engelmann. 1895.

APPENDIX III

Firms Recommended for the Supply of Psychological Instruments

The Instructor should secure the catalogues and price-lists of the following firms, — and of as many more as possible. A good collection of trade catalogues is indispensable to the economical conduct of a laboratory.

1. M. Bradley Co., Springfield, Mass. (Coloured papers, rings, etc.)
2. J. Brändli, 59 Freie Strasse, Basel. (Griesbach's instruments.)
3. Cambridge Scientific Instrument Co., St. Tibb's Row, Cambridge. (Optical and acoustical pieces; Galton's instruments.)
4. Clark University Laboratory, Worcester, Mass. (Sanford's instruments.)
5. Chicago Laboratory Supply and Scale Co., 31–45 W. Randolph Street, Chicago, Ill. (General supplies; Jastrow's instruments; certain of the instruments recommended in the text.)
6. Collin, 6 Rue de l'École de Médecine, Paris. (Dynamometers, etc.)
7. Columbia University Laboratory, New York City. (Cattell's instruments.)
8. C. Diederichs, Göttingen. (Müller's instruments.)
9. Eimer and Amend, 205 Third Avenue, New York City. (Chemicals; glassware.)
10. H. Elbs, 17 Friedrichstrasse, Freiburg i. B. (Münsterberg's instruments.)
11. J. H. Harting-Bank, Utrecht. (Zwaardemaker's instruments.)

12. R. Jung, Heidelberg. (Helmholtz' instruments.)
13. D. B. Kagenaar, Utrecht. (Instruments by Donders, Snellen, Engelmann, Zwaardemaker.)
14. R. Kœnig, 27 Quai d'Anjou, Paris. (Acoustical instruments: Helmholtz, etc.)
15. M. Kohl, 51 Poststrasse, Chemnitz i. S. (General supplies; optics, acoustics.)
16. F. Majer, 10 Krämergasse, Strassburg i. Els. (Ewald's instruments.)
17. E. B. Meyrowitz, 104 E. 23 Street, New York City. (Optics, acoustics.)
18. Michigan Apparatus Co., 305 South Main Street, Ann Arbor, Mich. (Lombard's and Pillsbury's instruments.)
19. Moore & Moore, 105 Bishopsgate Street, London, E. C. (Ellis' harmonical.)
20. W. Petzold, 13 Bayersche Strasse, Leipzig. (Physiological instruments: Ludwig, von Kries, etc.)
21. Prang Educational Co., 7 Park Street, Boston, Mass. (Spectrum chart; coloured papers.)
22. Queen & Co., 1010 Chestnut Street, Philadelphia, Pa. (General supplies; optics, acoustics.)
23. R. Rothe, 16 Liebigstrasse, Leipzig. (Hering's instruments.)
24. W. Schmidt, Seltersweg 30, Giessen. (Sommer's instruments.)
25. H. Sumner, Cambridge, Mass. (Bowditch's instruments.)
26. C. Verdin, 7 Rue Linné, Paris. (Physiological instruments: Marey, Mosso, etc.)
27. Yale University Laboratory, 109 Elm Street, New Haven, Conn. (Scripture's instruments.)
28. Ziegler Electric Co., 141 Franklin Street, Boston, Mass. (General supplies: optics, acoustics.)
29. E. Zimmermann, 21 Emilienstrasse, Leipzig. (Wundt's instruments.)

LIST OF MATERIALS

See Part I., pp. 207 ff.

I. SPECIAL APPLIANCES

II. GENERAL APPLIANCES AND MATERIALS

III. PAPER, DRAWING MATERIALS, ETC.

IV. SMELL AND TASTE SOLUTIONS, ETC.

V. LABORATORY FURNITURE

INDEX OF NAMES

This list does not repeat the names contained in the bibliography of the Geometrical Optical Illusions, pp. 305 ff.

INDEX OF SUBJECTS